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EDITED BY

J. MARK BALDWIN
JOHNS HOPKINS UNIVERSITY

HOWARD C. WARREN
PRINCETON UNIVERSITY

AND

CHARLES H. JUDD
YALE UNIVERSITY
(*Editor of the Psychological Monographs*)

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Communicated by Professor George M. Stratton

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The MSS. of these studies were revised November 8, 1908.

THE LOCALIZATION OF SOUNDS.

By Dr. Knight Dunlap.

Several years ago I commenced the attempt to make comparisons between the location of sounds with both ears and the location with one ear, the other being stopped up as well as might be. The results of my first tests were rather odd, showing a condition which made it impossible to get at the comparison I wished, at least in any clear way; and subsequent tests which I have made from time to time and which students have made for me, on different subjects, have resulted in the same way. The condition mentioned has had so little (if any) consideration in connection with the problem of the location of sounds, that I have thought it important to give some account of my experiments. For the purpose of illustration some data obtained in the last year are sufficient, since it is strictly typical of the mass I have earlier obtained.

My method of experimentation requires but a few words of explanation. I used first the Titchener sound cage, and later the Pillsbury cage. The telephone receiver of the Titchener cage was excited by an induction current produced by the usual method of induction coil, storage battery, and 100 d. v. fork. In other cases it was excited by making and breaking a storage battery current connected directly. The Pillsbury machine was used first with the buzzer with which it is provided, and subsequently with a Galton whistle. The data presented below were obtained with the Pillsbury cage and buzzer.

Both cages have the stimulus radius too short, so that it is not practicable to experiment much below one hundred and twenty degrees from the vertex. The Pillsbury cage came with scale reading in one hundred divisions of the circle, and had to be rescaled. Both machines are provided with clamps for holding the subject's head. These clamps, being attached to

the frame carrying the stimulus-arm, must not be used, for in even light contact with the head they conduct sound appreciably. In the experiments described here, the position of the subject's head was constantly watched to see that it was kept properly.

Stimulations were given from forty-eight points chosen on the sphere described by the stimulus-arm, in accordance with the diagram of Fig. 1. The subject shut his eyes at a warning signal before the stimulus-arm was swung into position, and opened them at a second signal, after the arm had been swung out of position. The positions were chosen haphazard by the experimenter, and checked off as given on a chart like Fig. 1. After opening his eyes the subject indicated the position of the sound as it seemed to him, and the experimenter marked that down on his chart. In some experiments the subject pointed with his forefinger to the apparent position of the sound, and the experimenter swung the stimulus-arm to the position to which the subject pointed, reading the same from the scale of the instrument. It was found, however, that in many cases the experimenter could not tell where the subject was trying to point, and in others the subject did not point where he thought he did. For this reason, in most of the experiments (including the ones from which the data below are drawn), a small chart was supported in front of the subject, on a rod rising from the floor, and the subject (having thoroughly oriented himself in the chart), indicated the position thereon. He was, however, asked to point the direction first, and the experimenter called his attention to any discrepancy in the two judgments, not suggesting correction but allowing the subject to correct in whichever way he saw fit. A high degree of accuracy was obtained in this way.

A complete series of forty-eight judgments was all that could be obtained at one sitting without wearying the subject. On different days series were taken, now with both ears, and now with the right ear or with the left ear. When one ear was used the other was stopped by the method which I have found most effective: a plug of vaselined cotton was pushed into the meatus of the ear, not tightly, the outer end of the plug well inside; then the outer end of the meatus and the concha were

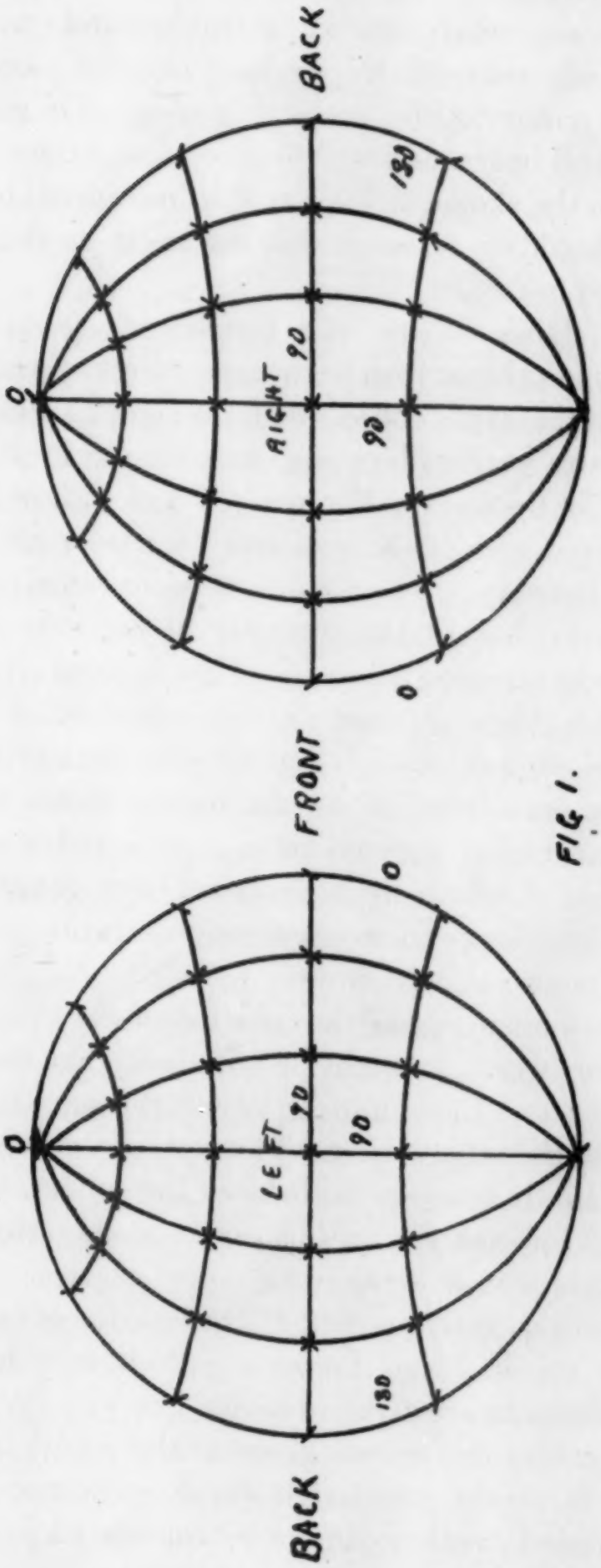


FIG 1.

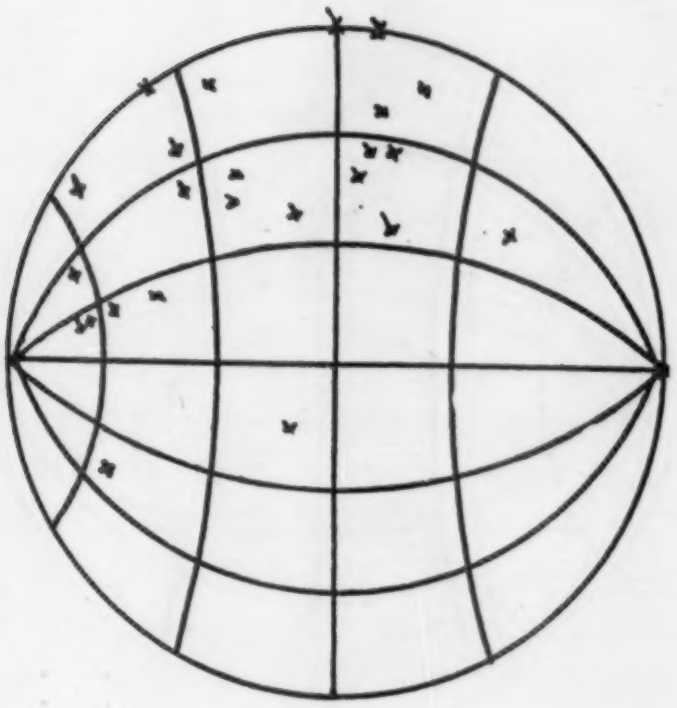
filled with thick paste. This deadened the ear more than could be done by any other method, with absolutely no discomfort, and was easily removed by washing out the paste with tepid water, and removing the protecting plug. Of course the ear so stopped still hears, and will hear to some extent (by conduction through the skull), as long as it is uninjured; but the sound employed should be so weak that the effect on the plugged ear is very slight indeed.

Figure 2 shows where the forty-eight stimulations were located by one subject with both ears. Figs. 3 and 4 show the locations by the same subject with the right and left ears respectively. Figure 8 shows how the same subject located with both ears three months later. Figures 5, 6 and 7 show how another subject located with both ears, and with the right ear and the left ear respectively. The conditions were exactly the same for all these cases, except the stopping of the ears as indicated. The first three series on subject S. (Figs. 2, 3 and 4,) and the three on subject D. (Figs. 5, 6 and 7), were obtained in January; the fourth series on subject S. (Fig. 8) was obtained the last of April. The peculiarity of all the results shows here at once. Each subject has at a given time a preferred position, to the neighborhood of which the sounds are very generally referred. The general tendency to increased concentration when only one ear is used is also clearly shown.

It is important to notice the location of the sounds from each part of the sphere. It might be supposed that the upper locations in the area of locations represent the sounds given in the upper part of the sphere; that the posterior locations of the group are of the sounds given in the back of the sphere, etc. The figures give no information on this point, as the attempt to introduce it there would make them into puzzles. The experimenter, however, kept a record of the location of each individual sound, and the locations for each meridian of longitude and parallel of latitude are given in tables I to VI.

In these tables the sounds given at the points in the median plane, that is, on the great circle dividing the two hemispheres, are not included[1], as they can not be considered in either hemi-

[1]Except insofar as they are given in Table VIII.



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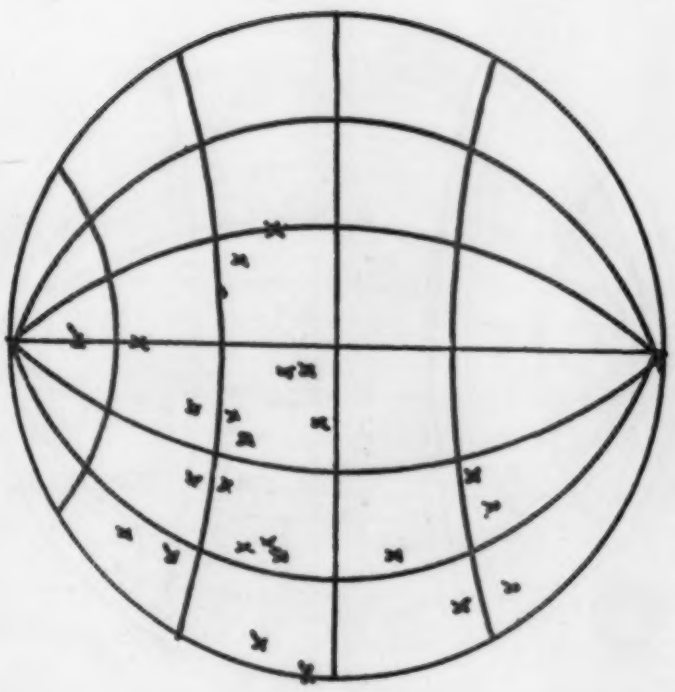


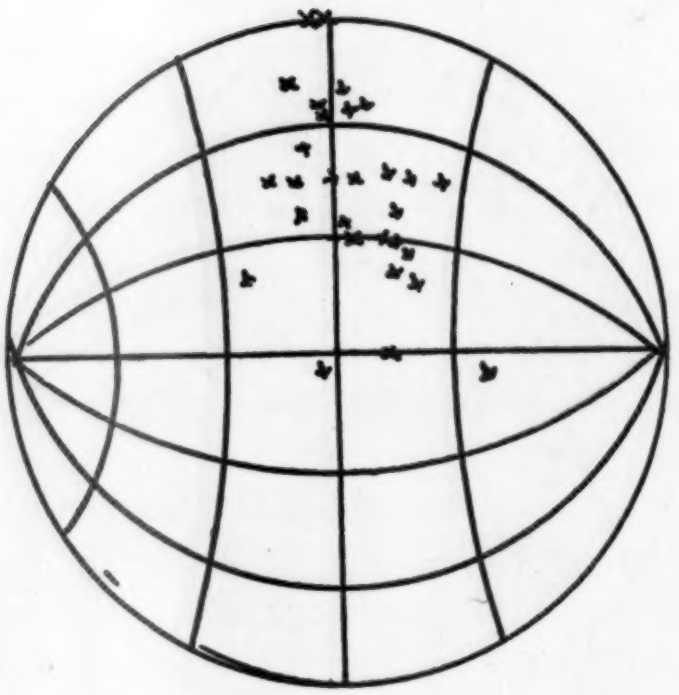
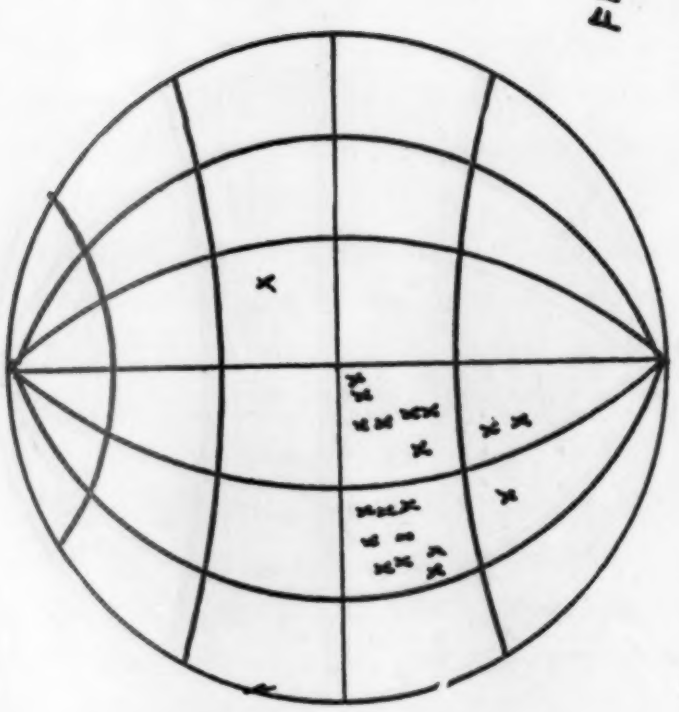
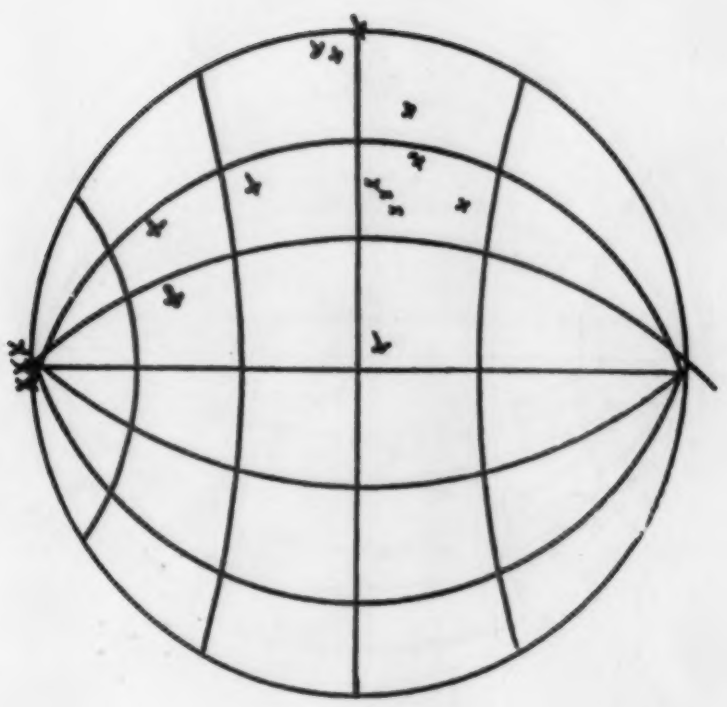


FIG. 8





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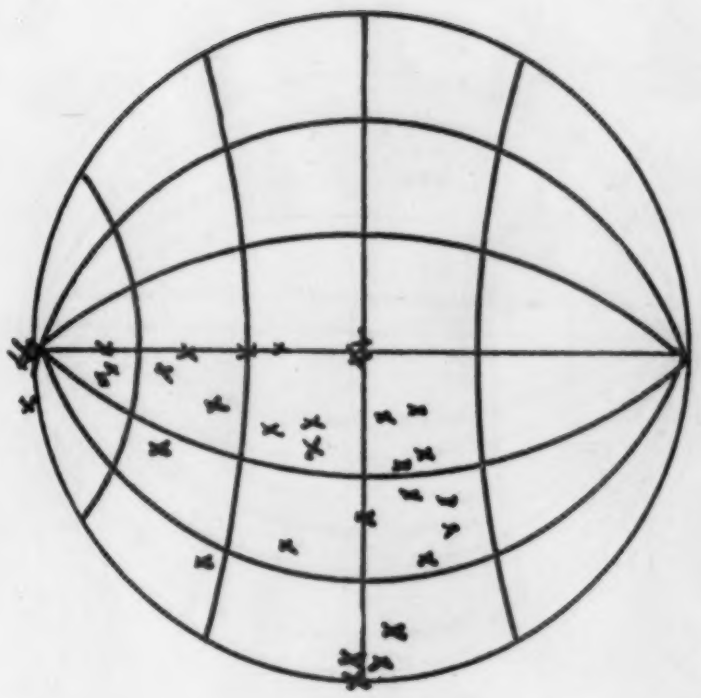

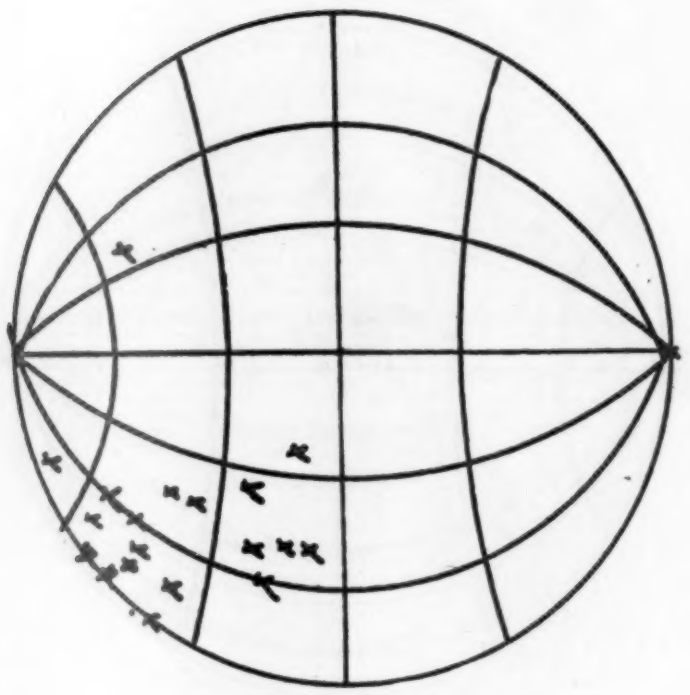

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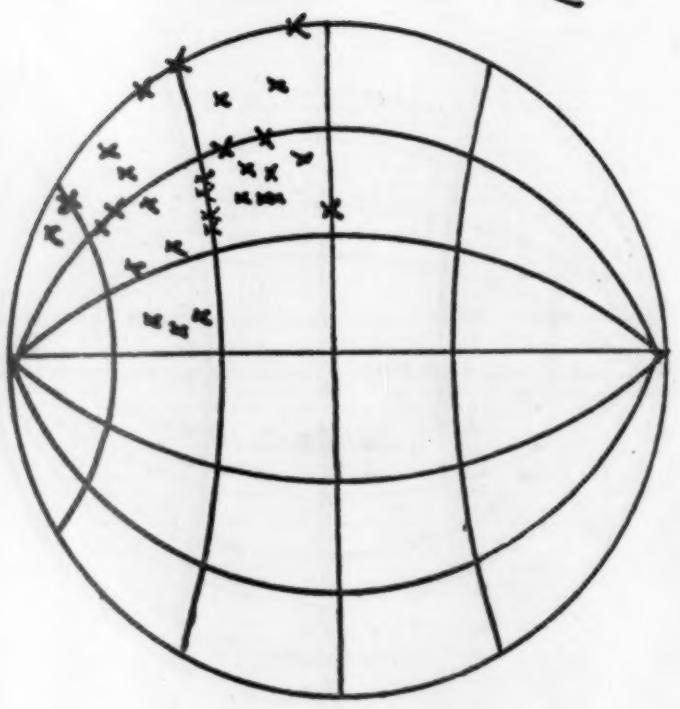

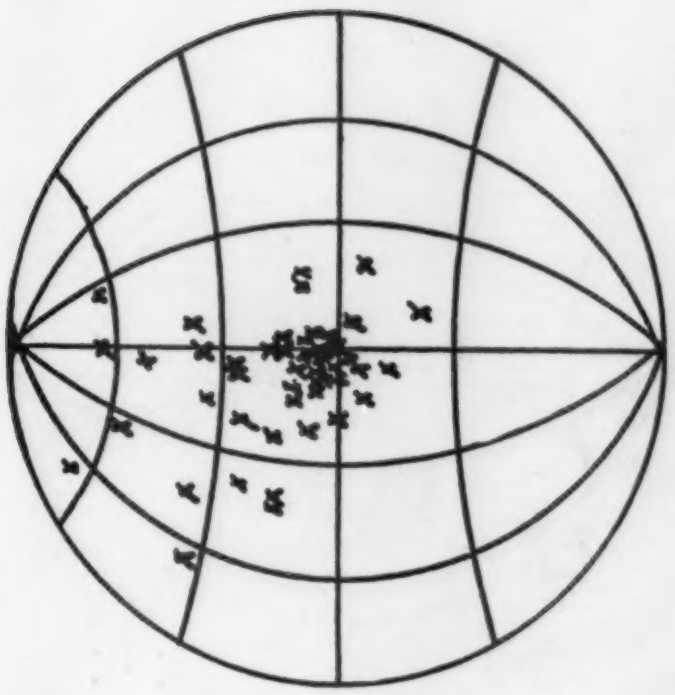

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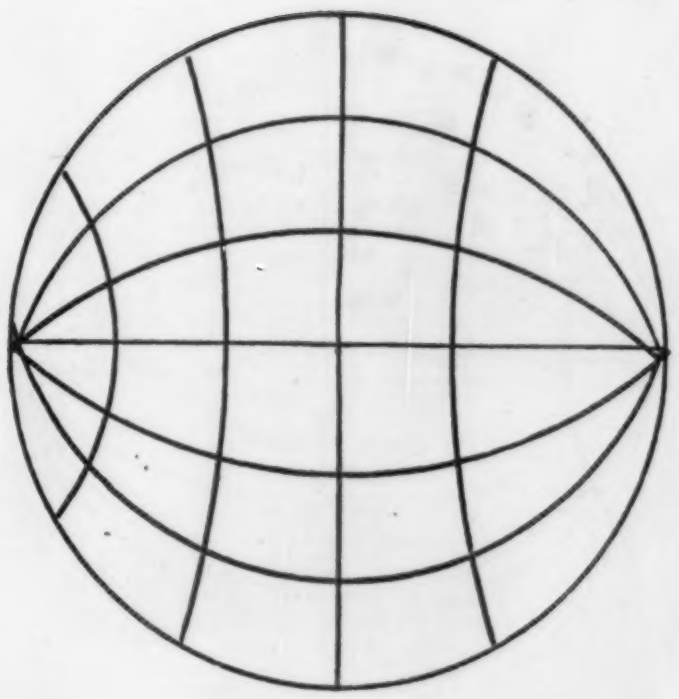

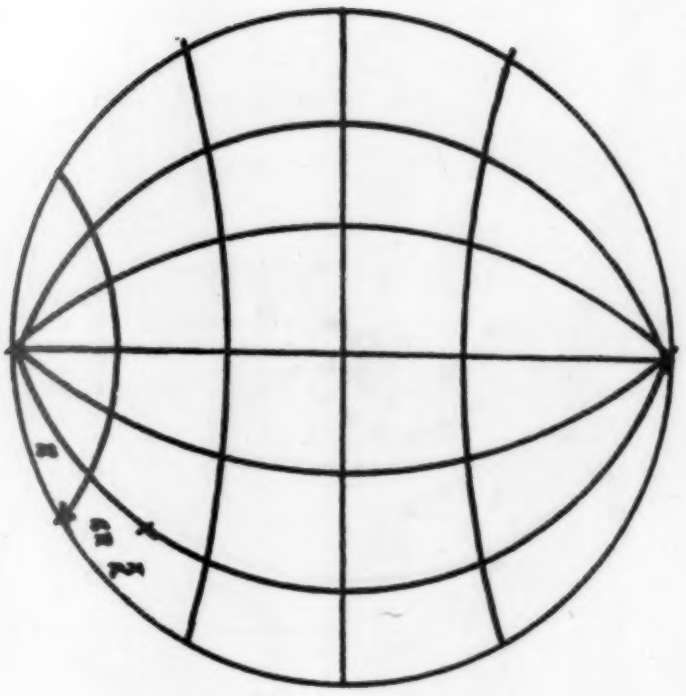

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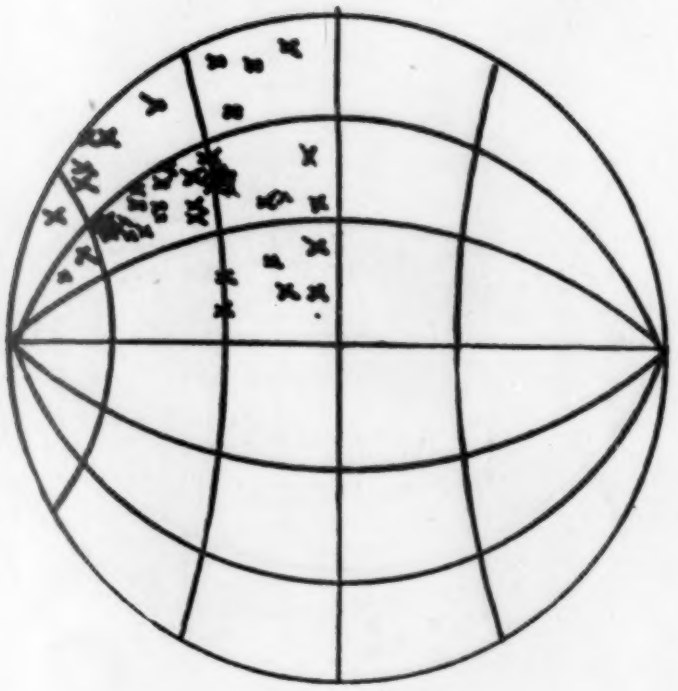

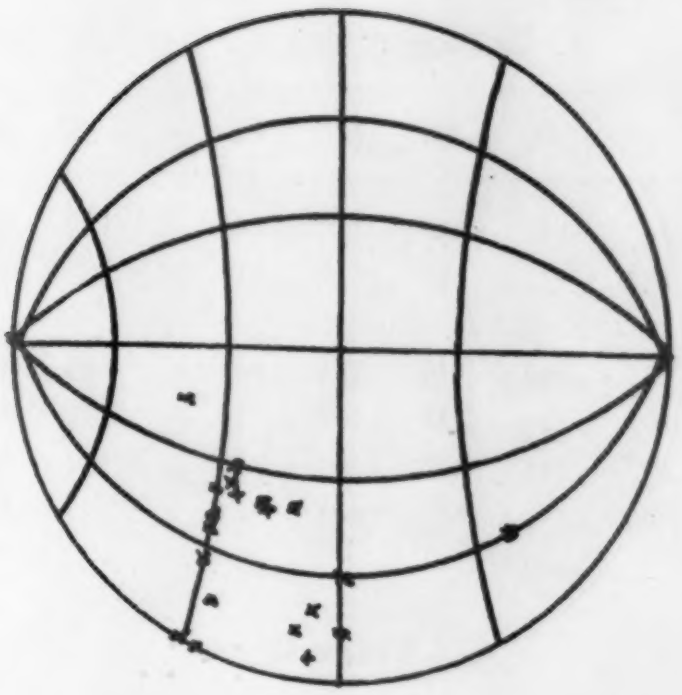

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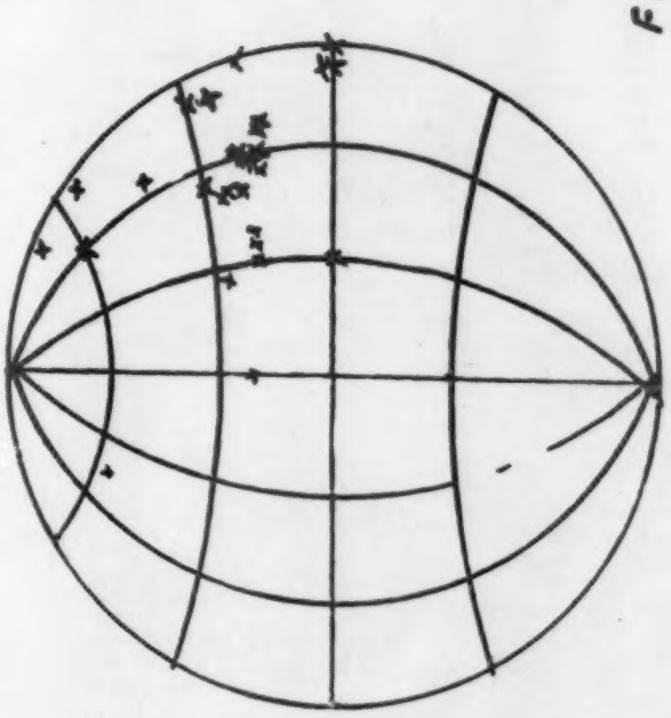

TABLE I.

Longitudes of Locations. Subject S.

		A. By Long. of Origin.					B. By Lat. of Origin.			
Ears	Hem.	30	60	90	120	150	30	60	90	120
Both	Rt.	141	155	132	136	141	138	124	154	149
	Lft.	132	131	140	138	122	138	125	125	142
Rt.	Rt.	127	143	116	135	142	145	145	124	118
	Lft.	102	143	128	108	115	127	109	122	121
Lft.	Rt.	138	141	128	103	110	151	120	126	100
	Lft.	110	137	117	125	121	133	102	124	126

TABLE II.

Longitudes of Locations. Subject D.

		A. By Long. of Origin.					B. By Lat. of Origin.			
Ears	Hem.	30	60	90	120	150	30	60	90	120
Both	Rt.	43	52	28	3	46	24	40	37	37
	Lft.	45	51	46	21	28	28	47	33	46
Rt.	Rt.	85	77	90	92	86	91	89	58	95
	Lft.	92	67	97	70	75	91	87	66	78
Lft.	Rt.	21	13	31	46	52	46	16	25	45
	Lft.	45	51	55	11	45	31	57	39	39

TABLE III.

Longitudes of Locations. Subject S.

		A. By Long. of Origin.					B. By Lat. of Origin.			
Ears	Hem.	30	60	90	120	150	30	60	90	120
Both	Rt.	37	42	50	17	17	24	27	46	37
	Lft.	18	41	58	66	36	26	49	63	39

TABLE IV.

Latitudes of Locations. Subject S.

		A. By Lat. of Origin.				B. By Long. of Origin.				
Ears	Hem.	30	60	90	120	30	60	90	120	150
Both	Rt.	36	50	99	77	93	65	48	65	55
	Lft.	68	63	122	97	73	102	102	90	68
Rt.	Rt.	84	96	101	106	105	83	97	95	102
	Lft.	109	95	99	103	100	105	96	102	103
Lft.	Rt.	68	58	78	51	92	50	66	26	68
	Lft.	40	59	99	89	87	70	47	61	92

TABLE V.

Latitudes of Locations. Subject D.

		A. By Lat. of Origin.				B. By Long. of Origin.				
Ears	Hem.	30	60	90	120	30	60	90	120	150
Both	Rt.	36	38	58	47	57	41	50	42	28
	Lft.	55	56	75	60	55	65	72	55	60
Rt.	Rt.	84	83	65	89	83	70	83	87	86
	Lft.	70	61	60	59	58	48	52	72	85
Lft.	Rt.	50	44	38	48	45	42	32	47	57
	Lft.	36	57	63	54	70	52	70	37	50

TABLE VI.

Latitudes of Locations. Subject S.

		A. By Lat. of Origin.				B. By Long. of Origin.				
Ears	Hem.	30	60	90	120	30	60	90	120	150
Both	Rt.	68	68	72	86	75	71	66	80	75
	Lft.	68	69	65	67	68	70	77	47	72

sphere. But their location differs in no way from that of the other positions, following the same rule. With this exception, the average latitudes and longitudes of location are given for the points on each parallel and meridian, so from the tables we may see the effects severally of the actual latitude and longitude of the sounds on the latitude and longitude of their location. The averages are strictly by position of origin, *i.e.*, by the positions where the sound was given, locations on the opposite hemisphere being taken at their measurements in that hemisphere. The fractions are dropped throughout the tables.

Table VII gives the average of all the locations of the sounds, first with reference to the hemisphere of origin, and second with reference to the hemisphere of location only. These averages were not taken from the figures of Tables I to VI, but from the same data from which those averages are drawn. Table VIII shows the number of sounds given in the median plane located on the right and left sides respectively, and the numbers given in the left hemisphere and located in the right and *vice versa*.

TABLE VII.

Averages of all Locations.

			By Hems.: A. Of Origin.				B. Of Location.	
Subject	(Tables)	Ears.	Hem.	Long.	a. v.	Lat.	a. v.	Long.	Lat.
S.	(I, IV)	Both	Rt.	141	20	65	28	132	68
			Lft.	133	21	87	28	137	84
		Rt.	Rt.	133	20	97	9	133	95
			Lft.	119	16	102	6	115	104
		Lft.	Rt.	125	24	64	30	136	71
			Lft.	121	20	71	29	118	66
D.	(II, V)	Both	Rt.	34	24	44	10	32	45
			Lft.	38	15	61	13	39	61
		Rt.	Rt.	85	10	81	10	83	71
			Lft.	81	16	62	18	..	..
		Lft.	Rt.	33	19	45	10	12	35
			Lft.	41	17	55	12	41	52
S.	(III, VI)	Both	Rt.	33	14	73	9	33	73
			Lft.	44	20	67	8	44	67

If all the sounds given in each hemisphere were located accurately where given, the average locus for each hemisphere would be Longitude 90 and Latitude 75, with average variations of 45 and 30 respectively. Dividing these variations by the average variations in Table VII gives the indices of concentration for the several cases.

TABLE VIII.

Locations from Median Plane and From Opposite Sides.

Subject.	(Tables)	Ears.	Med. to Rt.	Med. to Lft.	Lft. to Rt.	Rt. to Lft.
S.	(I, IV)	Both	4	4	0	1
		Rt.	5	3	7	3
		Lft.	1	6	0	8
D.	(II, V)	Both	2	6	0	4
		Rt.	8	0	20	0
		Lft.	1	7	0	15
S.	(III, VI)	Both	0	8	0	0

As a matter of fact, the position in the area of location bears precious little relation to the actual position of the sound. The marks representing the sounds at the various points might to all intents and purposes be shaken in a box and dumped down in the preferred area on the chart. This appearance is amply confirmed by other series on the subjects. Repeated series give results which have no uniformity except in the general area of location.

The preferred position is not determined by the character of the sound, or by the environment. Two subjects in exactly the same circumstances may have quite different preferences (Fig. 2 and Fig. 5). A subject may show the same preference after six months or a year, or may show a decidedly different one without any known reason for the change (as Subject S., Fig. 2 and Fig. 7). The subject shows the same preference in different rooms, or if he is reversed in the same room. Alterations in the intensity of the sound produce no definite alteration in the preferred area. The Galton whistle gives practically the same results as the buzzer or telephone receiver. Of

course, in any test the sound was kept as constant as was possible.

So far I have not found a subject who does not localize in this preferential way. What the causes are I can not say. There are possible theories, but nothing more. Meantime, how to conduct profitable experiments in localization before solving this problem is another problem.

CONCERNING FLUCTUATING AND INAUDIBLE SOUNDS.

By Dr. Knight Dunlap.

In a previous piece of work (Psych. Rev., XI, 1904, pp. 308–318), I found that interruptions in a faint sound could be correctly reported by an observer who at the time failed to perceive the sound itself. I found this phenomenon occurring both when the interruptions were made during the 'silent' intervals of a minimal sound which was undergoing the alternation of appearance and disappearance sometimes designated 'fluctuations of attention,' and also when interruptions were made in a sound which was at no time above the observer's threshold. These results were obtained by means of a telephone receiver actuated by the secondary current of a DuBois-Reymond induction coil, the primary current being furnished by a gravity battery and interrupted by a fork of 100 vibrations per second. The cessations were produced by breaking the secondary circuit, the primary circuit remaining undisturbed. Recently suspicions have been raised against these observations through consideration of certain peculiarities in the behavior of the diaphragm of the telephone receiver, and these suspicions merit attention and removal.

When the diaphragm of a telephone receiver is set in vibration by an electric current induced as above described, it oscillates *through* an average position which may be nearer the magnet than is the position of rest, or which may be farther from the magnet than is the resting position.

The change from the position of rest to the *average* position, when it occurs, may be readily observed. One convenient method, applicable when the change is produced by a moderate current, is to notice the approach or separation of two lines mirrored on the surface of the diaphragm, after the familiar

ophthalmoscopic method. Such observations show, and theory also requires, that the average position is nearer the magnet than is the position of rest, when the terminals of the magnet winding are so connected with the induction coil that the major phase of the current (namely, that caused by the *break* of the primary current), strengthens the magnetic field, and the lesser phase (caused by the *make* of the primary current), weakens the field. If the connections are reversed, so that the stronger current weakens the field, and the weaker current strengthens the field, we would expect to find, and do find clearly, that the average position of the vibrating diaphragm is farther from the magnet than is the position of rest, *unless* the current is very strong, in which case conditions are set up which we need not consider, since the statements just made apply to intensities up to those giving loud noisy sounds, whereas in the psychological experiments in question we use only feeble intensities.

By the introduction of a proper condenser in parallel with the fork (and a condenser ought always to be employed, if only in justice to the fork), this change of average position can be lessened, but probably can not be entirely obviated.

The conditions attendant on the use of the primary current for direct excitation of the receiver do not concern us, for that method of producing the sound is not advisable, and is not employed in experiments on minimal sounds.

The practical doubts raised by the foregoing observations are: (1) Does the release of the diaphragm from its forced vibrations through an average position different from the position of rest allow it to spring back to its normal position with a final movement of greater amplitude than the amplitude of its forced vibrations? and (2) Does the release and consequent springing back result in a movement of more rapid rate (and hence in a tone of different pitch) from that of the forced vibrations?

The first doubt can be readily dismissed. In fact it could arise only through a misconception of the mechanics of the diaphragm and magnet. Suppose for instance the average position of the diaphragm in movement is nearer the magnet than is the position of rest; if the current is discontinued while the diaphragm is in a part of its path nearer the magnet than

the resting position, it will move to the resting position against the normal field of the magnet, whereas under the conditions of the forced vibrations it moves from that same point towards the position of rest against only the weakened field of the magnet. Similarly, if the current is discontinued while the diaphragm is in a part of its path lying farther from the magnet than does the position of rest, it will move to the position of rest under the attraction of the normal magnetic field, whereas otherwise it would move to that position under the attraction of the augmented magnetic field. A corresponding condition is found when the average position is farther from the magnet than is the position of rest. When the current is discontinued, the diaphragm moves towards the position of rest either *against* a *stronger* magnetic pull or *with* a *weaker* pull than is the case while the current is supplied. There is therefore no final vibration of amplitude greater than those which have preceded. The apparent rise in intensity of the sound just as it is discontinued is psycho-physiological. This does not apply to conditions where direct current or heavy current is employed, as already stated.

The second doubt, namely, as to the possibility of a sound pitch or timbre different from that of the preceding sound, arising at the moment of discontinuance of the current, probably also arises from a misconception of the nature of the action of the induction current on the magnetic field of the telephone receiver. If we suppose the diaphragm to be released and spring back into place when the current is discontinued, it is easy to imagine it thereupon emitting its own note, which will usually differ from that previously emitted. Such an event may occur with the excitation of the magnet by the primary current, but not under the conditions we are considering, where the 'release' at the discontinuance of the current is less marked than that which occurs once in each complete phase of the exciting current. Here as before, we except from our consideration alternating currents whose opposing phase is strong enough not merely to weaken, but actually to *reverse* the polarity of the magnet.

The above arguments may not be convincing, and of course

it is possible that a more expert electrician will find that there are considerations which I have overlooked. I do not propose to rest my case here, but to show how the original experiments may be repeated and the results verified by means which are not open to the questions above discussed.

Since in many laboratories the only practicable means of sound transmission is by electrical transformation, it is worth while to consider methods of producing minimal tones from the telephone receiver. The necessity for our purposes is an alternating current of equal phases of uniform frequency and voltage. A small bell ringer would answer admirably, if driven by power under proper speed control, were it not for the fact that it has moving contacts, an arrangement absolutely fatal to the uniformity we require. This defect can be remedied however by having suitable windings put on the permanent magnets themselves, in which case the constancy of the current is governed solely by the constancy of the speed. Such a machine however, is useful only for low tones, since it is not safe to drive it at high speed. A special generator with permanent magnets and stationary windings giving higher frequency for low speed might be constructed without difficulty. The generator invented by Dr. Cahill for his telharmonium would be splendid if obtainable, since the wave form of its current approximates the cosine curve more nearly than that of any other generator.

A device much simpler than the troublesome generator, and one highly to be recommended, employs a pair of telephone receivers, one being used as receiver and the other as a generator. If the two are connected in circuit and one be brought near a source of sound, the sound is reproduced in the other distinctly, and with intensity which is not great at best, but is dependent on the proportions of the receivers. If as source of sound a singing gas flame be installed in one room, with one receiver near either end of the tube, and the other receiver be fastened near the observer's ear in another room into which the sound of the gas flame itself does not penetrate, we have a practical mechanism for the production and control of a steady tone. The constancy of the tone can be assured, after the flame has burned long enough to warm the tube, by maintaining the gas supply constant and excluding draughts.

The gas directly from the main *may* be used in certain places at certain hours of the day, provided an indicator is attached, and constantly watched to certify that no changes in pressure occur during the time the observations are made. Variations not visible in a water-level indicator will produce no consequential variations in the loudness of the tone when the flame is burning high. If the infinitesimal variation produced in the weak transmitted sound by minute variations in the loud original tone were noticed, the phenomenon would be more remarkable than any we are describing. The strongest confirmation comes from the fact that considerable observable variation in pressure produces no concomitant 'fluctuation' of the tone, even with a subject accustomed to note the 'fluctuation.' The necessary plan in most circumstances, and the safest in all, is to use an independent gasometer. 'Gas regulators' are of course worthless in work of this kind.

The intensity can be regulated in several ways. In the first place the distance from the receiver to the ear of the subject can be fixed to suit the needs of the case. In the second place, a metal disc can be adjusted above the flame-tube, so that the sound is damped to almost any degree. In the third place, a controlable induction apparatus may be placed in series or parallel with the two receivers. In the fourth place, the distance between the transmitter and the flame-tube may be changed.

If a tone purer than the 'noisy' complex tone of the gas flame is required, a resonator introduced in front of the transmitter serves the purpose admirably. The tone produced in the other telephone is now of striking roundness and purity. An adjustable resonator is preferable, for by mistuning it another simple means of intensity control is introduced. If the resonator is fixed in position, the transmitter may be moved away from it with the result that the intensity alone is modified, or practically so, since the flame may be so regulated, that the effects of the sound waves other than those transmitted through the resonator are negligible. If no resonator is used, any change in position of the transmitter modifies the timbre of the tone on account of the standing waves which the gas flame sets

up, even in a large room. Damping the sound by lowering a disc over the top of the tube always changes the timbre if no resonator is employed.

It is no doubt suggested by the description, and will be apparent on trial, that the manipulation of the apparatus, simple though it is, requires large patience and that there are a number of cautions to be observed. It is however possible to perform the manipulation successfully, and obtain intensities of sound which manifest the characteristic fluctuations to observers capable of noticing them. A much simpler arrangement is to attach a rubber tube to the resonator and convey the sound directly to the other rooms, provided the building is equipped with a sound pipe or you have no scruples about drilling holes in the wall through which to run your rubber tube. In this last case the intensity may be controlled by a screw-compress on the tube, or the tube may be cut and a cock inserted. A collecting funnel might be used on the end of the tube, in place of the resonator, if the simplicity of the tone is not an object. I have not tried that arrangement, however.

The gas flame with resonator and either method of transmission furnishes the best available means for demonstrating the fluctuations of musical tones, and the only simple means at present for performing the experiment with a *nearly* simple tone.[1] For the interruption experiments referred to at the beginning of this paper, either form may be used, with or without the resonator. I have used the resonator in both cases. In the case of electrical transmission the circuit may be broken, and this is of course the simplest method. The phases of the current being symmetrical the diaphragm of the second telephone vibrates symmetrically through the point of rest, and the discontinuance of the current can thinkably cause only a speedy cessation of vibration. However, there is another method which does not break the current abruptly, but reduces it

[1] Whistles similar to the Stern 'Tonvariators' might be used if the constancy of the note could be assured. I have not been able to make them give a constant note. If used, such whistles should be blown loudly by air at constant pressure, and the intensity of the note reduced in transmitting; for they are more steady when loudly blown.

rapidly, entirely (it seems to me), disarming criticism. This method is to bring a card laterally over the anterior opening of the resonator (not touching the resonator). This progressively lowers the pitch of the resonator, lessening its response therefore, and practically silencing it at the end of the movement. It goes without saying that in using this method the resonator should in the first place be tuned to, or a little below, the pitch of the gas flame note, and that the lowest strong partial of the flame must be used. If the upper partial is used the lowering of the pitch of the resonator may 'bring in' the lower note. But, really, with a proper resonator, the lower note will excite the resonator tuned down to it in this way so feebly that this caution may not be needed.

When using direct air transmission, the just-described method of discontinuing the sound may be employed, but is rather needless. The simpler method of turning the stop-cock is perfectly adequate. Another method is to have the tube cut in a room intervening between that containing the gas flame and that containing the observer, and the ends secured just far enough apart to permit a card to be passed between them without touching. In this case the intensity may be regulated from this room by the compression method (any screw clamp will serve). I have obtained good results by placing the resonator in a room separated by a wooden partition from the room containing the flame, running the tube from the resonator into a third room protected by a non-conducting brick and cement wall.

In any of these forms the perception of the cessation of an inaudible sound may be demonstrated as readily as the fluctuations in a just perceptible tone. In both cases considerable practice in careful observation is usually required (though this is by no means always the case) and some individuals are totally unable to notice the fluctuations in any tone, and others only in certain easy cases.[1]

[1] For practice or other general work on attention, I would especially recommend the water-dropping device described in *Science*, XXVI (1907), 247–8, since it is exceedingly simple and convenient of manipulation, the intensity of the sound being regulated by placing the plate on which the drops fall at the

The failure to notice fluctuations in minimal tones is really due to inability to notice fine distinctions in the tones. The observer is simply unable to notice whether the tone is present or not, and hence adheres constantly to an arbitrary judgment. This sounds like a reckless assertion, but is supported by strong evidence. If an observer either hears the sound continuously, or fails to hear it at all, and no intensity can be found at which he notices the fluctuations, you can readily find intensities at which he will go on hearing the sound for long periods after it is absolutely cut off, provided you cut it off rather gradually. He really is incompetent to distinguish between the presence or absence of the sound of that particular intensity. This sort of blundering is characteristic of those who are apparently constitutionally unable to notice the fluctuations, and also of those who simply have not yet had sufficient practice. Moreover, almost any observer is liable to relapse into this condition at times. The nicely discriminating subject, on the other hand, who clearly notices the fluctuations, can not be caught that way. However carefully the suppression is made he very shortly reports it. He does not make his judgment instantly, unless the sound is cut off abruptly; and in fact the most careful subjects make no pretense to record, or even notice the exact time of the disappearance or reappearance of the sound when physically constant but 'fluctuating,' simply recording the judgment that at the indicated time the disappearance or reappearance had taken place; but he is distinctly not to be caught napping. So uniform are the results of these check experiments in the cases of those subjects whose accuracy I would be inclined to grant on other grounds, that I place no reliance on the results of experiments with those who do report fluctuations, and are yet flagrantly caught by the check experiment. Observers who fail to notice accurately the presence or absence of the weak sound need not be caught if they abandon the attempt and report no sound unless it is above this intensity, for there is

proper distance below the dropper. By using a rapid rate of drop, and a highly resonant object for it to strike on (e.g., a hollow metal box), a *practically* continuous sound may be secured, with which the interruption experiment may be performed, interposing a piece of thick felt to catch the drop when desired.

no check to show they hear when they judge they do not. This however would argue a prejudice on the part of the observer, or a lack of proper attitude towards the task, which fortunately I have not encountered. A large part of the difficulty is obviated, of course, if the observer does not know of the check experiments.

A NEW KEY FOR REACTION-TIME WORK.

By Dr. Knight Dunlap.

The imperfection of the ordinary telegraph form of the reaction key has long been recognized. As a result of this recognition we have had a number of modified forms of the key, such as Jastrow's, Bergstrom's magnetic, and Scripture's sliding key, which are designed to simplify the attention required of the reactor, and which in some measure succeed. Experimenters who have not found these keys satisfactory, or who have concluded *a priori* that they are not satisfactory, have been led to employ the release form of reaction, with which the telegraph form of key is *mechanically* satisfactory, or else to discard keys altogether, and employ other mechanisms, such as air bulbs.

The chief source of trouble in the ordinary key with the 'press'-reaction, is the unintentional breaking (or closing) of the circuit before the stimulus is given. When the measurements are made by the tuning-fork and drum method the circuits may be so arranged that such accidents do not come to the notice of the reactor, and hence no harm is done beyond the loss of time and reactions, which under these circumstances is large. But if the reactor becomes aware of these faults, as he must in the ordinary arrangements of apparatus, the disturbance is much more serious; for he inevitably endeavors to avoid them and so complicates his attention and lessens the facility of the reaction. This is especially true with the so-called sensory attention; many reactors have told me that they were unable to put the fingers on the key ready to react, and then completely withdraw the attention from finger and key, because the fingers are so apt to press a little too strongly or to be raised from the key. If the fingers do maintain their position it is through a cramping of the muscles which impedes the reaction. Others find little difficulty in this particular; and of course it tends to

disappear with practice, as the fingers become a more and more automatic mechanism, adjusting themselves to the key.

Since much experimentation must be done with relatively unpracticed reactors (for in certain work it is just the reactions of the relatively unpracticed person which are important); and since in any case the distraction due to the key may be important even if the reactor should not think it so; it is important to have a key requiring the least possible attention. None of the improved keys so far introduced reduce this attention factor sufficiently without the use of an initial resistance so high as to be a disturbing element in itself. The Jastrow and Bergstrom keys are in no wise better than an ordinary key with a spring

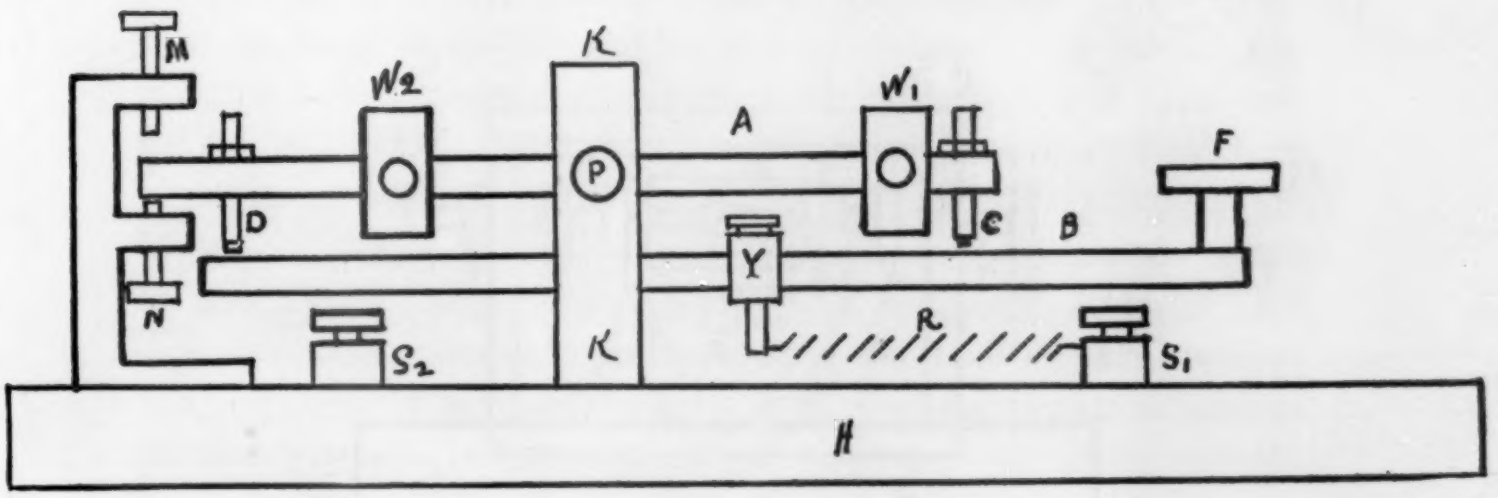

Fig. 1.

tension as strong as the initial resistance of one of these. The ideal mechanism would be the pneumatic bulb, were it not for the difficulties of long-distance air transmission. If the reactor could be in the same room with the recording apparatus, and if the kymographic method were always practicable, we would seek no farther.

In the attempt to construct an electric key having the advantages of the pneumatic system, I designed the key which is represented in Figures 1 and 2, and had it constructed by Mr. Childs, mechanician of the Physics Department of Johns Hopkins University. The instrument is there drawn to scale, the actual length of lever *A* being six inches. To allow for alterations if such should be needed, the proportions were purposely

made such that the instrument appears clumsy. A second key would be made much more compact.

On the three-ply wood base *H*, the rectangular steel frame *KK* is fastened, the lower side countersunk flush. The screws *P*, pass through the frame and are fastened by the lock-nuts *T*. The small rectangular frame *E-E-E* is tapped for screws *R* with lock-nuts *L*, the screws being hollowed at the tip for the cone points of the spindle *V*, and hollowed in the heads for the cone points of the screws *P*. The frame *E-E-E* and the spindle *V* therefore turn independently on adjustable cone bearings.

The spindle *V* carries the lever *A*, which is bored for it. The lever *B* is screwed to the frame *E-E-E*, but insulated from it by

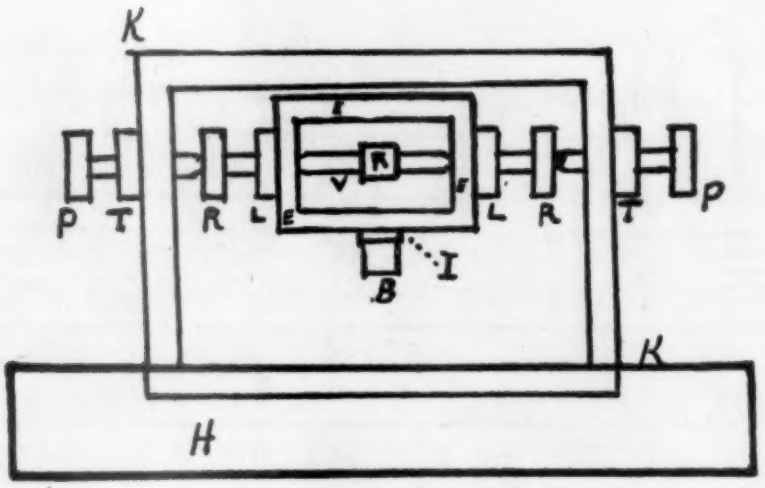

Fig. 2.

I. *F* is a hard rubber finger button, *M* and *N* are stops for the lever *A*, and *S* and *S* are stops for the lever *B*. The coil spring *R*, which holds up the *F* end of lever *B*, is adjustable by sliding the clamp *Y*. The set screw on *Y* is at the top, but will be changed to the side to be entirely out of the way. *W* and *W* are weights sliding on lever *A*, and clamped by set screws. *C* is a platinum-tipped screw, provided with lock-nut; and *D* is a screw, similar, but with an insulating hard rubber tip. *C* and *D* are interchangeable. The wires from the battery are led to levers *A* and *B* through binding posts and connections not shown in the cuts.

The weights are so adjusted that the upper lever exercises sufficient pressure on either *D* or *C* (whichever is desired) to follow the lower lever in slow movements, but has sufficient inertia to allow the lower lever to break away from it in a quick movement. The stops are so adjusted that when lever *B* is in extreme position up or down, contact is permanently made or broken (whichever is desired). Therefore, make or break by the beginning of a reaction-movement is maintained permanently as long as desired.

The key as represented is adjusted for break-circuit, 'press': (the adjustment of the contact and stops is not represented in the cut with absolute accuracy, of course). By sliding the weights so as to balance the lever in the opposite direction the key can be adjusted for make-circuit, 'release.' By leaving the weights as represented and interchanging pegs *C* and *D*, it can be adjusted for make-circuit, 'press.' By making both the changes mentioned, it may be adjusted for break-circuit, 'release.' Finally, by clamping the lever *A* fast between stops *M* and *N*, it becomes an ordinary, or telegraph form of key. This last possibility is important in making comparison between the new form of key and the old; for by keeping the same spring-tension nothing is changed except the factor under investigation.

The key works perfectly, except in the make-circuit, release. In this case there is a rebound, as there is with an ordinary telegraph key so used. This adjustment of the key has not been needed so far, but if it becomes necessary to use the key in this way (make-circuit, 'release'), a simple attachment, already planned, will perfect it. No loss of time between the beginning of the movement of the finger lever and the breaking of the contact is discoverable (by the spark method), with any fairly slow voluntary movement, when the weights are properly adjusted. It is almost impossible to make, when trying to do so, a single movement which shall give any interval. There is absolutely no loss therefore with the abrupt movements of reaction. Yet the unintentional movements (increase or decrease in pressure) of the reagent while attentively expecting the stimulus, occur without interrupting the contact. Such

interruptions as are made when the reagent is putting his fingers on the key, at the warning signal, make no difference, as the current should not be turned on at that time.

In practice, the key fulfills its intention. Accidental breaks of the circuit (in break-reactions) never occur, and the ease with which the subject can withdraw attention from the hand and key altogether is remarkable. No attempts have been made yet to find how low a reagent can bring the 'sensory' reaction with brief practice on that alone, but some tests have been made with both 'sensory' and 'motor' attention to compare the reaction with the new key with the reaction with the ordinary key (*i.e.*, with the lever *A* held firmly between the stops *M* and *N* as described above). Results with the two reactors with whom extended tests were made are given in Tables I to X.

These experiments were made with the Hipp Chronoscope, checked constantly with the tuning fork. The stimulus was the click of a telephone receiver held in the regular operator's headpiece. The room in which the reactor sat was constantly illumined faintly by a small gas flame, and the warning signal was the turning out of an electric light, enclosed in a box with a ground glass aperture which illuminated the room strongly enough so that the reactor did not have to look at the ground glass. The light was turned on from three to five seconds, approximately, before it was turned out, giving the reactor plenty of time to put his hand on the key. When the signal light was out, after a reaction, the reactor was completely at rest; a condition which cannot be attained where the warning is a click or bell or flash, or the turning off of a light which is turned on again immediately after the reaction. In these latter cases the reactor is continually on the strain of expecting the warning signal or else is surprised by it in many cases, and hence the attention is not normal when the stimulus arrives. The warning signal (extinction of the major illumination) was given about two seconds before the stimulus. Exactness in this interval was of course purposely avoided. The interval finally seemed a little too long; one and three-fourths seconds would have been better; but with visual warning perhaps a little longer time is needed than when the warning is auditory.

The reactors were given the following instructions, in practically the following terms: "In the 'motor' series, put the fingers on the key while the light is on. Then as soon as the light goes off, attend strictly to the movement you are to make, with the intent of making it as quickly as possible when the proper time comes, dismissing the sound altogether, for it will do its work without your attention. In the 'sensory' series, place your fingers on the key while the light is on. Then as soon as the light goes off put your attention strictly on the sound you expect, with the intent of hearing it as quickly as possible, dismissing your hand and the movement altogether, for they will take care of themselves." All reactors are very soon able to carry out these instructions, although nearly all agree that the 'sensory' attention is the more easily attained.

Two series, one motor and one sensory, were taken on the same day. The reactor was taken out of his room for ten minutes or so, between the series, for exercise and diversion; yet there is no doubt that the first interfered with the second. This seemed to be a necessary evil, if both sensory and motor series were to be taken.

The results of the first series taken with the automatic key on two reactors are given in Tables I and II. In these and the following tables the first column gives the number of the pair of series (from the first pair), the second column gives the number of reactions in the 'sensory' series, the third column the average, the fourth the average variation, and the fifth the median. The sixth, seventh, eighth and ninth columns give the number of reactions, average, average variation, and median for the 'motor' series, and the tenth gives the order of the pair, e.g., 'M' signifies that the motor series came first.

Both of the reactors had had one period of practice in reacting, about a week before the first series recorded. At the beginning of each sensory and each motor series three reactions were taken without starting the chronoscope. No other practice was given during the progress of the experiment.

After the completion of the sets detailed in Tables I and II, sets were taken with the upper lever clamped as before described, the instrument being practically converted into a key of the

telegraph type. The results of these sets are given in Tables III and IV, under the caption of 'Plain Key.' With reactor B a third set was taken after the completion of the second, the key being 'automatic,' as in the first set. Results of this are given in Table V.

This work was scattered over a period of two months, in the case of each reactor.

TABLE I.

Reactor S. Automatic Key.

	Sensory	Av.	m. v.	Med.	Motor	Av.	m. v.	Med.	
1	(19)	144.78	25.28	146.	(19)	127.26	25.75	130.	M.
2	(20)	142.5	36.6	124.	(20)	138.85	33.52	125.	S.
3	(20)	134.3	22.2	134.5	(20)	135.25	33.8	127.	M.
4	(20)	131.65	28.78	136.5	(20)	117.75	24.33	128.5	S.
5	(23)	129.86	20.8	125.	(23)	134.04	27.33	138.	S.
6	(35)	88.17	26.38	80.	(35)	81.57	18.10	81.	S.
7	(37)	80.59	18.90	73.	(37)	89.90	30.37	80.	M.

TABLE II.

Reactor B. Automatic Key.

	Sensory	Av.	m. v.	Med.	Motor.	Av.	m. v.	Med.	
1	(15)	193.8	38.74	199.	(15)	156.6	52.98	135.	M.
2	(20)	157.05	25.35	157.	(20)	130.35	32.28	119.5	S.
3	(20)	97.	12.3	90.	(20)	94.45	18.9	83.5	M.
4	(35)	122.31	37.36	120.	(35)	119.42	58.22	97.	S.
5	(35)	99.74	17.23	97.	(35)	91.6	13.71	90.	M.
6	(32)	84.28	18.09	80.	(32)	90.25	29.45	78.	S.

TABLE III.

Reactor S. Plain Key.

	Sensory	Av.	m. v.	Med.	Motor.	Av.	m. v.	Med.	
1	(40)	128.75	22.3	122.5	(40)	99.17	21.17	95.5	M.
2	(38)	127.63	21.92	125.5	(38)	113.93	23.22	115.5	S.
3	(30)	126.86	24.65	120.5	(30)	125.83	20.71	127.5	M.
4	(30)	114.73	15.86	113.5	(30)	138.90	28.16	129.5	S.

TABLE IV.

Reactor B. Plain Key.

	Sensory	Av.	m. v.	Med.	Motor.	Av.	m. v.	Med.	
1	(30)	133.46	16.91	125.	(30)	128.56	22.53	120.5	M.
2	(35)	115.28	15.42	110.	(35)	110.05	17.73	102.	M.
3	(30)	123.96	20.09	120.5	(30)	148.3	41.85	134.	S.
4	(30)	110.96	24.38	93.5	(30)	92.23	17.36	84.	S.
5	(35)	104.11	18.74	99.	(30)	109.08	21.95	98.	S.

TABLE V.

Reactor B. Automatic Key.

	Sensory	Av.	m. v.	Med.	Motor.	Av.	m. v.	Med.	
1	(30)	95.73	14.16	92.	(30)	93.23	18.66	84.5	M.
2	(30)	96.01	17.33	92.	(30)	103.66	17.66	97.5	S.
3	(30)	102.76	22.70	97.	(30)	111.03	17.91	103.	M.
4	(30)	109.86	10.56	107.5	(30)	118.16	20.13	108.5	S.

These tables sum up the results of all the reactions, none being excluded, however unusual, except those which the reactor himself indicated currently as given during distraction; and such indications were characteristically few, although both reactors would allege after the completion of a series, that a number of the reactions were probably not good, because of disturbance by noises in another part of the building, etc. This reluctance on the part of a relatively inexperienced subject to cancel reactions or observations is always a favorable sign.

The rejection of data by the experimenter is in any case an arbitrary matter, and yet a few measurements which deviate markedly from the rest of a series ought not to be allowed to obscure the tendency manifested by the rest. That is to say, after presenting the figures for *all* the data, as we have done above, figures should be presented which represent the data from which exceptional cases have been excluded, provided the excluding is done with strict impartiality.

The most practical method of exclusion proceeds from an inspection of the data after arrangement in order of magnitude

for determination of the median. Different rules of procedure will be called for by different sorts of data. In the case in hand it was seen that by cutting out the lowest time in all series, the two highest in series ranging up to twenty reactions, and the highest four from series of twenty-five or more reactions, most of the exceptional times were eliminated. These exclusions were therefore made, and the averages computed on the basis of the reactions retained. The figures are given in Tables VI to X.

TABLE VI.

Reactor S. Automatic Key. (Table I.)

	Sensory	Av.	m. v.	Med.	Motor.	Av.	m. v.	Med.
1	(16)	140.75	19.4	144.	(16)	120.25	17.62	121.
2	(17)	134.41	24.64	123.	(17)	125.41	16.64	121.
3	(17)	129.23	16.09	133.	(17)	124.11	19.75	126.
4	(17)	128.64	23.66	135.	(17)	115.88	20.25	117.
5	(20)	126.95	16.35	122.	(20)	129.15	21.3	135.
6	(30)	80.36	16.4	77.	(30)	85.93	10.2	86.5
7	(32)	74.	10.75	72.	(32)	82.19	23.65	76.

TABLE VII.

Reactor B. Automatic Key. (Table II.)

	Sensory	Av.	m. v.	Med.	Motor.	Av.	m. v.	Med.
1	(12)	185.41	21.20	189.	(12)	147.41	21.23	132.
2	(17)	151.88	15.57	155.	(17)	118.46	19.95	119.
3	(17)	94.34	10.	90.	(17)	91.58	14.39	83.
4	(30)	114.23	16.51	113.5	(30)	103.13	35.93	92.
5	(30)	99.63	11.93	94.5	(30)	87.6	10.15	90.
6	(27)	77.92	10.67	78.	(27)	79.77	12.83	78.

TABLE VIII.

Reactor S. Plain Key. (Table III.)

	Sensory	Av.	m. v.	Med.	Motor.	Av.	m. v.	Med.
1	(35)	123.66	21.88	121.	(35)	93.07	14.87	91.
2	(33)	119.81	14.7	122.	(33)	112.	21.27	102.
3	(25)	110.72	11.57	111.	(25)	129.48	19.65	127.
4	(25)	118.	15.36	120.	(25)	120.6	15.56	125.

TABLE IX.

Reactor B. Plain Key. (Table IV.)

	Sensory	Av.	m. v.	Med.	Motor.	Av.	m. v.	Med.
1	(25)	123.19	8.27	124.	(25)	118.08	8.	117.
2	(30)	109.63	8.8	109.	(30)	104.66	9.7	101.
3	(25)	118.08	11.67	119.	(25)	135.04	29.44	121.
4	(25)	99.52	15.88	93.	(25)	88.2	12.48	85.
5	(30)	99.63	12.51	97.5	(30)	99.	14.34	97.

TABLE X.

Reactor B. Automatic Key. (Table V.)

	Sensory	Av.	m. v.	Med.	Motor.	Av.	m. v.	Med.
1	(25)	91.6	8.3	91.	(25)	84.29	8.50	93.
2	(25)	89.72	9.14	89.	(25)	97.4	9.88	96.
3	(25)	93.	10.32	93.	(25)	97.48	10.48	97.
4	(25)	105.	5.88	107.	(25)	109.76	11.35	109.

The reductions in the mean variations justify the exclusions made; and this really is the only possible justification of any arbitrary exclusions. When the rejection of a few members of a series produces a comparatively large shrinkage in the mean variation, we are justified in considering the excluded members as really exceptional. The only use we wish to make of the reduced tables, however, is to show that the relations of the unmodified tables are not accidental. This is shown by the substantial agreement of the two sets of tables in the relative values; what we have to say below will hence apply to either group of tables.

The effects of practice are clearly shown in the first two tables. Not until the last two series did either reactor get down to a stable point. The fourth table shows improvement in the use of the plain key, but the third table shows no improvement in the plain-key series of S. It must be remembered that the series were so scattered over a long period of time that no great amount of practice-improvement could be expected. What reduction in time occurs in the first five series of each reactor is due to the most general sort of habituation to the conditions

of the experiment. The alternation of motor and sensory attention in each experiment hour was a disturbing factor, and no idea can be gained from these tables of what a number of consecutive days practice on either type of attention would produce.

The mean variation is somewhat high, because of the features just mentioned; but it is always high in reaction time measurements with little practice. A review of the standard literature of reaction times discloses the great unwillingness of the majority of the experimenters to reveal exactly what their mean variations were.

The motor reaction-time is not universally shorter than the sensory. On this point some interesting observations occurred, confirming some suspicions which had been raised before. With 'sensory' attention, there is apt to be more or less division of the attention, if the reactor is not cautious; as the finger must be put in position on the key and kept there. With 'motor' attention the reactor is apt to keep his attention on the key until just about the time for the stimulus, and then shift it to the stimulus; this of course is the best form of sensory attention, and if the reactor calls this 'motor,' his so-called 'sensory' attention is pretty sure to be 'mixed.' I have no doubt that this is the case whenever reactors show consistently a motor reaction time considerably less than their sensory time. It is very easy for even an experienced subject to make these errors if his method of directing the attention is not carefully prearranged and retrospective criticism not carefully employed.

The tables show an apparent advantage of the automatic key over the plain. Under the conditions of the experiment no more could be expected. The spring of the instrument was adjusted to the best tension for the plain key, which was not the best for the automatic. The subject became so familiar with the tension of the spring during the experiments with the automatic key, that he was in the best possible condition for experiments with the plain key later.

That the automatic key gives greater speed, even under indifferent conditions, there can be no doubt. These observations have been confirmed by briefer series on three other subjects,

which agree entirely with the conclusion. Still other series, on two subjects, with sensory attention only, show a more striking advantage for the automatic key. The superiority of the new key results partly from increased facility in withdrawal of attention from the hand in the sensory series, and obviation of the cramping of the muscles in the motor, giving a freer movement in each case.

The key was designed and built to be used (if satisfactory) in an experiment, now in preparation, on an hitherto untouched point in reaction-times. The tests of this instrument have been so successful that it will be employed as planned.

THE INFLUENCE OF THE VARIATION OF WEIGHT UPON THE JUDGMENT OF EXTENT.

By William Wilberforce Costin, Ph.D.

In the study of illusions it is sometimes found that the experiment may be reversed and the result still be an illusion. It is interesting to note that the well known illusion of Aristotle in which the deception of touch is brought out—as when a pencil is placed between the crossed index and second finger and two pencils are felt to be there instead of one—has its converse, as seen in the fact that two objects quite a distance apart when touched by the crossed fingers will produce an impression similar to that habitually produced by a single object. The fact of the converse of Aristotle's illusion led to the question of the possibility of the converse to what is known as the weight-size illusion.

In the weight-size illusion the size clearly affects the apparent weight. For instance, equal weights (50 gr.) are compared while concealed in two boxes of different sizes. It is found, by lifting, that the weight in the larger box seems lighter than that in the smaller when they are in fact equal. The sight of the larger box leads the mind, guided by experience, to judge that it is heavier than the smaller. "But the actual weight of the larger box is so much less than the mind anticipates, and so much less than the hand is prepared to raise, that by contrast it seems much lighter than it otherwise would."[1] This illusion suggested the question as to what in some respects is, and in some respects is not, its converse, namely, what effect the variation of weight would have upon the judgment of extent.

In preparing for the experiment the following materials were employed: Rectangular blocks of rosewood were selected, cut and finished with constant dimensions in cross-section 22 mm. × 30 mm., but with lengths running from 42 mm. to

[1] Stratton, Experimental Psychology, p. 98.

54 mm. by steps of 1 mm. Thirteen blocks in all constituted the series. They were loaded invisibly with small shot and paraffine until each weighed 50 grammes. In addition to these there were three blocks called standards all of the same dimensions, namely, 22 mm. × 30 mm. × 48 mm. but of the following weights: 50 gr., 15 gr., and 85 gr.

Two methods were used in the experiment. The first the method of minimal changes. The second that of right and wrong cases, with some modification.

In the use of the first method all the blocks were employed, the series alternating from 42 mm. to 54 mm. throughout the experiment, and the three standards likewise. The subject, blindfolded, takes a position on the opposite side of the table from the experimenter, seated with his right side to the table. With his arm lengthwise with the table he holds his hand ready to lift each block, beginning with the standard, with its ends between the thumb and the second and third fingers.

Placed between the standard and the subject, half an inch away, was the first variable of the series. At the word 'ready' the subject reached forward and took the standard between his thumb and second and third fingers and lifted it from six to eight inches high. Then putting it down he took up the variable in the same way and gave a judgment as to whether the second in comparison with the first was longer, shorter or equal, or whatever his judgment might be. In this manner the whole series was gone through, both down and up, making twenty-six judgments in all for that standard. Then the 15 gr. and the 85 gr. standards were used in the same way. Seventy-eight judgments constituted one 'sitting.' This was all that was required of the subject in one day. Altogether there were seven subjects and each had five 'sittings,' a total of two thousand seven hundred and thirty judgments. Much care was taken to vary the order of series. Each subject in five sittings, was given the following variations in the standards employed: (1) 15 gr., 50 gr., 85 gr.; (2) 85 gr., 15 gr., 50 gr.; (3) 50 gr., 15 gr., 85 gr.; (4) 50 gr., 85 gr., 15 gr.; (5) 15 gr., 85 gr., 50 gr.

In going up and down the series from No. 1 to 13 and 13 to 1, the order was varied, in each sitting, and from sitting to sitting.

The following shows the form of the record kept for the judgments of each subject.

TABLE A.

STANDARD. WEIGHT, 50 GR. LENGTH, 48 MM.				STANDARD. WEIGHT, 15 GR. LENGTH, 48 MM.				STANDARD. WEIGHT, 85 GR. LENGTH, 48 MM.			
Length of variable in mm.	Judgment.	Length of variable in mm.	Judgment.	Length of variable in mm.	Judgment.	Length of variable in mm.	Judgment.	Length of variable in mm.	Judgment.	Length of variable in mm.	Judgment.
54		42		42		54		54		42	
53		43		43		53		53		43	
52		44		44		52		52		44	
51		45		45		51		51		45	
50		46		46		50		50		46	
49		47		47		49		49		47	
48		48		48		48		48		48	
47		49		49		47		47		49	
46		50		50		46		46		50	
45		51		51		45		45		51	
44		52		52		44		44		52	
43		53		53		43		43		53	
42		54		54		42		42		54	

Results by the Method of Minimal Changes.

In order to estimate the results of this method the various judgments were tabulated as follows:

The 'calculated equality-point' was determined by taking the length of the block at which the judgments 'longer' stop, and also of that at which the judgments 'shorter' stop, and taking their arithmetic mean. The difference between the two values which enter into such a mean is stated, under the heading 'zone of doubt.' The results thus obtained from each series of 13 'lifts' is put in the table as a separate entry; and at the bottom the average of these and the median. Under V are entered the variations of the 'equality point' from the average.

The following are the tables for all the subjects:

TABLE I.

Subject Ba.

STANDARD 50 GR.			STANDARD 15 GR.			STANDARD 85 GR.		
Calculated equality point.	V.	Zone of doubt.	Calculated equality point.	V.	Zone of doubt.	Calculated equality point.	V.	Zone of doubt.
mm.		*mm.*	*mm.*		*mm.*	*mm.*		*mm.*
49.5	.4	1	47.5	1.3	1	52.	2.45	2
49.5	.4	1	49.5	.7	1	50.5	.95	1
49.5	.4	1	47.5	1.3	1	50.5	.95	1
48.5	.6	1	50.5	1.7	1	49.5	.05	1
49.5	.4	1	47.5	1.3	1	48.5	1.05	1
49.5	.4	1	50.5	1.7	1	31.5	1.95	1
48.5	.6	1	50.5	1.7	1	46.5	3.05	1
46.5	2.6	1	48.5	.3	3	48.5	1.05	1
51.5	2.4	1	47.5	1.3	1	50.5	.95	1
48.5	.6	1	48.5	.3	1	47.5	2.05	1
Ave'ge 49.1	.88	1.	48.8	1.16	1.2	49.55	1.35	1.1
Median 49.5			48.5			50.		

TABLE II.

Subject Bu.

STANDARD 50 GR.			STANDARD 15 GR.			STANDARD 85 GR.		
Calculated equality point.	V.	Zone of doubt.	Calculated equality point.	V.	Zone of doubt.	Calculated equality point.	V.	Zone of doubt.
mm.		*mm.*	*mm.*		*mm.*	*mm.*		*mm.*
50.5	1.45	3	49.5	2.1	1	49.	.35	2
50.5	1.45	3	50.	2.6	2	50.	1.35	2
48.	1.05	2	49.	1.6	2	48.5	.15	3
49.5	.45	1	48.5	1.1	3	49.	.35	2
47.	2.05	2	47.	.4	2	49.	.35	2
47.	2.05	2	42.	5.4	4	47.5	1.15	1
49.5	.45	3	46.	1.4	2	48.5	.15	3
47.5	1.55	1	47.5	.1	.1	48.5	.15	3
51.	1.95	2	47.	.4	2	47.5	1.15	2
50.	.95	2	47.5	.1	2	49.	.35	1
Ave'ge 49.05	1.34	2.1	47.4	1.52	2.1	48.65	.55	2.1
Median 49.5			47.5			48.75		

TABLE III.
Subject C.

STANDARD 50 GR.			STANDARD 15 GR.			STANDARD 85 GR.		
Calculated equality point.	V.	Zone of doubt.	Calculated equality point.	V.	Zone of doubt.	Calculated equality point.	V.	Zone of doubt.
mm.		*mm.*	*mm.*		*mm.*	*mm.*		*mm.*
48.	.1	4	47.	.45	2	49.5	.9	3
47.5	.4	3	46.	1.45	2	48.	.6	2
48.	.1	2	49.	1.55	2	49.	.4	2
49.	1.1	4	49.5	2.05	5	49.	.4	2
48.	.1	2	46.	1.45	2	48.5	.1	3
48.5	.6	7	47.5	.05	3	49.	.4	2
47.5	.4	3	48.5	1.05	3	47.5	1.1	3
48.	.1	4	45.5	1.95	3	47.5	1.1	3
47.	.9	4	47.	.45	4	49.	.4	2
47.5	.4	1	48.5	1.05	7	49.	.4	2
Ave'ge 47.9	.42	3.4	47.45	1.15	3.3	48.6	.58	2.4
Median 48.			47.25			49.		

TABLE IV.
Subject D.

STANDARD 50 GR			STANDARD 15 GR.			STANDARD 85 GR.		
Calculated equality point.	V.	Zone of doubt.	Calculated equality point.	V.	Zone of doubt.	Calculated equality point.	V.	Zone of doubt.
mm.		*mm.*	*mm.*		*mm.*	*mm.*		*mm.*
52.	2.6	2	47.5	.35	9	50.5	.55	5
49.5	.1	5	45.5	2.35	7	49.	.95	4
48.5	.9	3	47.	.85	8	50.	.05	4
48.5	.9	1	49.	1.15	2	50.	.05	4
49.	.4	8	47.	.85	8	49.	.95	4
50.5	1.1	3	48.	.15	6	50.	.05	6
49.5	5.	3	50.	2.15	4	50.	.05	2
47.	2.4	6	49.5	1.65	5	50.5	.55	5
49.	.4	2	47.5	.35	5	51.5	1.55	1
50.5	1.1	1	47.5	.35	7	49.	.95	2
Ave'ge 49.4	1.	3.4	47.85	1.02	6.1	49.95	.57	3.7
Median 49.25			47.5			50.		

TABLE V.

Subject F.

STANDARD 50 GR.			STANDARD 15 GR.			STANDARD 85 GR.		
Calculated equality point.	V.	Zone of doubt.	Calculated equality point.	V.	Zone of doubt.	Calculated equality point.	V.	Zone of doubt.
mm.		*mm.*	*mm.*		*mm.*	*mm.*		*mm.*
52.	3.85	6	46.5	.6	3	50.5	1.8	9
48.	.15	4	47.	.1	2	47.5	1.2	5
49.	.85	6	45.	2.1	2	50.	1.3	6
46.5	1.65	3	46.	1.1	4	47.	1.7	6
50.	1.85	6	46.	1.1	2	48.5	.2	1
46.	2.15	2	49.5	2.4	1	45.5	3.2	3
48.5	.35	1	46.5	.6	1	46.5	2.2	1
47.5	.65	1	49.5	2.4	1	50.5	1.8	1
46.5	1.65	1	46.5	.6	1	51.5	2.8	1
47.5	.65	1	48.5	1.4	1	49.5	.8	1
Av'age 48.15	1.38	3.1	47.1	1.24	1.8	48.7	1.7	3.4
Median 47.75			46.5			49.		

TABLE VI.

Subject S.

STANDARD 50 GR.			STANDARD 15 GR.			STANDARD 85 GR.		
Calculated equality point.	V.	Zone of doubt.	Calculated equality point.	V.	Zone of doubt.	Calculated equality point.	V.	Zone of doubt.
mm.		*mm.*	*mm.*		*mm.*	*mm.*		*mm.*
50.	.0	2	54+	2.85	1	54+	3.05	1
50.5	.5	1	53+	1.85	2+	47.5	1.95	8
50.5	.5	3	50	1.15	2	51.5	.55	5
51.	1.	4	50	1.15	2	49.5	1.45	1
51.5	1.5	3	53	1.85	2	51.	.05	2
47.	3.	4	49.5	1.65	7	50.5	.45	1
50.	.0	2	53	1.85	2	52.5	1.55	1
47.5	2.5	3	51.5	.35	1	50.	.95	4
51.	1.	2	48.5	2.65	1	50.5	.45	1
51.	1.	4	49.	2.15	2	51.	.05	2
Av'age 50.	1.1	2.8	51.15	1.75	2.2	50.95	1.05	2.6
Median 50.5			50.75			50.75		

TABLE VII.

Subject W.

STANDARD 50 GR.			STANDARD 15 GR.			STANDARD 85 GR.		
Calculated equality point.	V.	Zone of doubt.	Calculated equality point.	V.	Zone of doubt.	Calculated equality point.	V.	Zone of doubt.
mm.		*mm.*	*mm.*		*mm.*	*mm.*		*mm.*
51.5	3.1	1	48.	.5	2	46.5	1.9	1
47.5	.9	3	48.5	.0	3	47.5	.9	1
47.5	.9	1	48.5	.0	1	48.5	.1	1
46.5	1.9	1	49.5	1.0	3	49.5	1.1	1
48.	.4	2	48.	.5	2	48.	.4	2
48.	.4	2	48.	.5	2	48.5	.1	1
46.5	1.9	1	48.	.5	2	49.5	1.1	3
48.5	.1	2	48.	.5	2	48.	.4	2
48.5	.1	1	49.	.5	4	50.	1.6	2
51.5	3.1	3	49.5	1.0	1	48.	.4	2
Av'age 48.4	1.28	1.7	48.5	.5	2.2	48.4	.8	1.6
Median 48.			48.25			48.25		

Tables Explained.

Subject Ba, Table I.

Taking the averages under 'calculated equality point,' the judgments of subject Ba were such that the block weighing 50 gr. (the weight of all in the series) appeared (as we may estimate) to be 49.1 mm. long, whereas in fact it was only 48 mm. long. That is, the estimated length made it 1.5 mm. longer than the block really was. This difference was not the effect of weight, for the standard and the other blocks were all of the same weight. It was rather the effect of the 'constant-error,' due to the make-up of the subject plus the influence of the order of the series, etc. Now take the effect of using standard 15 gr., 48 mm. long. The same subject estimated on the average that the standard was .8 mm. longer than it really was. The question is: Did the lightening up of the standard affect the estimate

of the length? If we suppose the 'constant-error' the same as before, namely, the tendency to make 49.1 mm. seem equal to 48 mm. when both weigh 50 gr., then the lightening of the standard to 15 gr. had the effect of making it seem 48.8 mm. long, whereas the 50 gr. standard made it seem 49.1 mm. long—a difference of .3 mm. That is, the lightening of the standard made this standard seem .3 mm. shorter than the length at which the 'constant-error' placed it.

When the 85 gr. block was used as the standard the average was 49.55 mm. That is, the standard seemed .45 mm. longer than the length at which the 'constant-error' placed it. By taking the median lengths instead of the average the differences of judgments became slightly more pronounced. For instance the median with the 50 gr. block as standard was 49.5 mm. That of the 15 gr. standard was 48.5 mm., a lengthening of .5 mm. The outcome is to the effect that to lighten an object seems to reduce its length, and to weight down an object seems to increase its length.

The results with subject Bu indicate that to lighten an object decreases its length and to weight down an object does the same thing. In the case of subject C, to lighten an object decreases the length and to weight down an object increases the length. Subject D's results show that to lighten an object seems to reduce its length and to weight down an object increases its length. The results of subject F indicate that to lighten an object decreases its length and to weight down an object increases its length. In the case of subject S, to lighten an object increases its length and to weight down an object does the same thing. Subject W's results show that to lighten an object perhaps slightly increases its length and to weight down an object has little or no effect upon the estimate of its length.

From this method—that of minimal changes—the following summary statement may be made, namely, that while the results of subjects S and W indicate that to lighten up an object seems to increase its length, and subject Ba's results show that to weight down an object seems to decrease its length, and the results of subject W indicate no effect at all from weighting, yet by taking all seven subjects together the aggregated aver-

ages show that to lighten up an object tends somewhat to decrease its length and to weight down an object slightly increases its length.

The following is the table of aggregated averages and medians:

AGGREGATED AVERAGES AND MEDIANS.

Subject.	STANDARD. WEIGHT, 50 GR. LENGTH, 48 MM.				STANDARD. WEIGHT, 15 GR. LENGTH, 48 MM.				STANDARD. WEIGHT, 85 GR. LENGTH, 48 MM.			
	Calculated equality point. (Average.)	M. V.	Zone of doubt.	Calculated equality point. (Median.)	Calculated equality point. (Average.)	M. V.	Zone of doubt.	Calculated equality point. (Median.)	Calculated equality point. (Average.)	M. V.	Zone of doubt.	Calculated equality point. (Median.)
Ba	49.1	.88	1.	49.5	48.8	1.16	1.2	48.5	49.55	1.35	1.1	50.
Bu	49.05	1.34	2.1	49.5	47.4	1.52	2.1	47.5	48.65	.55	2.1	48.75
C	47.9	.42	3.4	48.	47.45	1.15	3.3	47.25	48.6	.58	2.4	49.
D	49.4	1.	3.4	49.25	47.85	1.02	6.1	47.5	49.95	.57	3.7	50.
F	48.15	1.38	3.1	47.75	47.1	1.24	1.8	46.5	48.7	1.7	3.4	49.
S	50.	1.1	2.8	50.5	51.15	1.75	2.2	50.75	50.95	1.05	2.6	50.75
W	48.4	1.28	1.7	48.	48.5	.5	2.2	48.25	48.4	.8	1.6	48.25
	48.85	1.05	2.5	48.92+	48.32+	1.19+	2.7	48.03+	49.25+	.94+	2.4+	49.39+

Results by the Method of Right and Wrong Cases.

In order to check the results obtained by the method of minimal changes the method of right and wrong cases with some modification was used and the following four of the same group of subjects were chosen, namely, Bu, C, D and S. The same standards were employed and three blocks from the series were selected being respectively 50 mm., 48 mm., and 46 mm. in length. Nine cards were prepared with the following combinations written upon them; it being understood, of course, that the weight given is that of a block 48 mm. long:

50 gr. 46 mm.	15 gr. 46 mm.	85 gr. 46 mm.
50 gr. 48 mm.	15 gr. 48 mm.	85 gr. 48 mm.
50 gr. 50 mm.	15 gr. 50 mm.	85 gr. 50 mm.

These cards were shuffled; then the combinations were placed permanently in the record in the order in which they happened to come up. For instance in the case of C the following is an illustration of the order in one shuffling:

Gram. Wt. of standard 48 mm.	Mm. length of variable wt. 50 gr.
85	50
15	46
15	50
50	50
50	48
15	48
85	48
50	46
85	46

There were ten sets of these group combinations of nine, each set being arranged by an entirely independent shuffle, making in all ninety judgments which constituted one 'sitting' —all that was required in one day.

There were five 'sittings' for each subject, that is, four hundred and fifty judgments for each, or one thousand eight hundred judgments in all.

The blocks were offered to the subject as the shuffling of the cards called for. As before, the judgments of longer, shorter, equal, etc.—a judgment describing the second block in comparison with the first block as standard—were made after lifting in succession the two blocks.

For any one 'sitting' the following table will illustrate the distribution of judgments for the different combinations of blocks:

Pair Compared	Judgments 'Shorter'	Judgments 'Equal'	Judgments 'Longer'
50–46	8	1	1
50–48	1	7	2
50–50		1	9
15–46	6	4	
15–48	2	6	2
15–50			10
85–46	7	3	
85–48	3	7	
85–50	1	4	5

The following tables give the distribution of judgments for each subject for the five sittings:

Subject Bu.

	'Shorter'					'Equal'					'Longer'					'Equal or Longer'	'Equal or Shorter'
50–46	8	9	7	8	9	1	1	3	2	1	1						
50–48	1	1	2	1	4	7	8	6	7	6	2	1	2	2			
50–50						1	1	4	2	6	9	9	6	8	4		
15–46	6	9	7	4	7	4	1	2	4	2			1	2	1		
15–48	2	2		1		6	8	8	4	8	2		2	5	2		
15–50							2		3	1	10	8	10	7	9		
85–46	7	10	8	9	8	3		2	1	2							
85–48	3	3	3	6	3	7	6	7	4	7		1					
85–50	1			1		4	5	6	5	9	5	5	4	4	1		

Subject C.

	'Shorter'					'Equal'					'Longer'					'Equal or Longer'	'Equal or Shorter'
50–46...	9	9	7	6	7		1	3	1	2	1			3	1		
50–48...	2	3		1	1	3	5	4	4	6	4	2	6	5	3	1	
50–50...						1	2	2		2	9	8	8	10	8		
15–46...	3	3	2	2	3	5	6	5	6	5	2	1	3	2	2		
15–48...	1		1		1	1	3	2		1	8	7	7	10	8		
15–50...	1						1		1		9	9	10	9	10		
85–46...	10	8	8	10	9		2	2		1							
85–48...	6	4		4	4	3	4	10	5	3	1	2		1	3		
85–50...	1	1		1		5	2	7	2	2	4	7	3	7	8		

Subject D.

	'Shorter'					'Equal'					'Longer'					'Equal or Longer'	'Equal or Shorter'
50–46...	10	8	9	7	7		1		2	2		1	1	1	1		
50–48...	8	3	4	3	5	1	2	3	3	4	1	5	3	4	1		
50–50...	5	2	1	2	2	1	1	1			4	7	8	8	8		
15–46...	5	2	7	4	5	2	2	3	2	1	3	6		4	4		
15–48...	3	1	4	2	4	1		1	3	3	6	9	5	5	3		
15–50...	2		2	2					1	1	8	10	8	7	9		
85–46...	10	8	9	8	10		1	1	1			1		1			
85–48...	9	9	7	4	6	1				2		1	3	6	2		
85–50...	7	6	3	5	2	1	2	3	1	4	2	2	4	4	4		

Subject S.

	'Shorter'					'Equal'					'Longer'					'Equal or Longer'	'Equal or Shorter'
50–46...	7	7	7	7	8	3	3	2	2	1				1	1		1
50 48...	5	8	5	4	4	4	1	3	5	3		1	2	1	3	1	
50–50...	3	2		1	1		4	3	4	2	6	4	7	5	6	2	
15–46...	8	10	9	8	10	2			2				1				
15–48...	9	7	4	8	7	1	2	2	2	3		1	4				
15–50...	5	1	1	3	3	1	3	5	3	3	4	6	4	4	4		
85–46...	9	9	8	7	5	1		1	3	5		1	1				
85–48...	7	9	5	5	3	3		4	4	7		1	1	1			
85–50...	5	3				3	6	4	3	4	1	1	6	7	6	1	

Combining them in summary the results are as follows:

Subject Bu.				Subject C.					Subject D.				Subject S.					
	'Shorter'	'Equal'	'Longer'		'Shorter'	'Equal'	'Longer'	'Equal or Longer'		'Shorter'	'Equal'	'Longer'		'Shorter'	'Equal'	'Longer'	'Equal or Longer'	'Equal or Shorter'
50–46	41	8	1	50–46	38	7	5		50–46	41	5	4	50–46	36	11	2		1
50–48	9	34	7	50–48	7	22	20	1	50–48	23	13	14	50–48	26	16	7	1	
50–50	0	14	36	50–50	0	7	43		50–50	12	3	35	50–50	7	13	28	2	
15–46	33	13	4	15–46	13	27	10		15–46	23	10	17	15–46	45	4	1		
15–48	5	34	11	15–48	3	7	40		15–48	14	8	28	15–48	35	10	5		
15–50	0	6	44	15–50	1	2	47		15–50	6	2	42	15–50	13	15	22		
85–46	42	8	0	85–46	45	5	0		85–46	45	3	2	85–46	38	10	2		
85–48	18	31	1	85–48	18	25	7		85–48	35	3	12	85–48	29	18	3		
85–50	2	29	19	85–50	3	18	29		85–50	23	11	16	85–50	8	20	21	1	

In order to estimate the results of this method let us examine the judgments of any subject from the above table, say subject S. Sixteen times he estimated block 48 mm. 'equal' in length to standard 50 gr. (48 mm.); twenty-six times he said it was 'shorter' than the standard, and seven times that it was 'longer,' and once he judged it either 'equal or longer'. It is evident that the 'constant-error' so influenced the estimate as to cause the subject to say twenty-six times that block 48 mm. was 'shorter' than the 50 gr. standard when they were actually 'equal' in length. Now take the effect of lightening the standard to 15 gr. If the lightening has no effect the result should approximate 26 'shorter,' 16 'equal,' 7 'longer' and 1 'equal or longer,' but as a matter of fact it stands 35 'shorter,' 10 'equal,' and 5 'longer.' What has been the effect of lightening? Evidently this, that nine judgments have been put into the 'shorter' column, and two have been taken from the 'longer' column. The lightening of the object thus seems to have had the effect of increasing its apparent length.

Now take the case where the standard is weighted down to 85 gr. If the weighting has no effect on the length we should expect to find the result approximately 26 'shorter,' 16 'equal,' 7 'longer' and 1 'equal or longer.' But in fact we find it 29 'shorter,' 18 'equal' and 3 'longer.' That is 4 'longer,' and 1 'equal or longer' have been distributed into the 'equal' and 'shorter' columns increasing these to 18 and 29 respectively. This result indicates that increasing the weight increased the apparent length.

Taking block 46 mm. and the 50 gr. standard, the 'constant-error' distributes the judgments as follows: 36 'shorter,' 11 'equal,' 2 'longer' and 1 'shorter or equal.' Comparing that with the result when the standard is lightened, the result stands 45 'shorter,' 4 'equal' and 1 'longer.' Nine judgments have been put in the 'shorter' column, indicating again that to lighten the object tends to increase its length.

For the other subjects it is only necessary to give the general conclusions. Subjects Bu, C and D, all show the same general result, namely, that to lighten an object is to decrease its apparent length, and to weight down an object is to increase

its apparent length. Subject S reversed this conclusion so far as lightening the standard is concerned, in this and the method of minimal changes also.

Subject Bu's judgments by this method of right and wrong cases come out somewhat different from those obtained by the method of minimal changes. By the method of minimal changes his judgments show that to weight down decreases the apparent length; but by this method—right and wrong cases—his results indicate that to weight down increases the length, *i. e.*, his judgment here accords with those of the main group of subjects.

The following is a summary of the results of all the subjects according to both methods:

METHOD OF MINIMAL CHANGES.

Subject.	Effect of decreasing the weight.	Effect of increasing the weight.
Ba	decrease of apparent size	increase of apparent size
Bu	" " "	decrease " "
C	" " "	increase " "
D	" " "	" " "
F	" " "	" " "
S	increase " "	" " "
W	" " "	seems not to affect size

METHOD OF RIGHT AND WRONG CASES.

Subject.	Effect of decreasing the weight.	Effect of increasing the weight.
Bu	decrease of apparent size	increase of apparent size
C	" " "	" " "
D	" " "	" " "
S	increase " "	" " "

It is evident, taking both methods together, that for most persons, and after making allowance for the constant error due to the succession in comparison, to lighten up an object seems to decrease its length and to weight down an object seems to

increase its length. We thus have an illusion in tactual space-perception that in some respects is, and in some respects is not, the converse of the weight-size illusion.

The exact converse of the weight-size illusion would be something as follows:

The illusion would start from the experience which the different weights arouse. Experience has taught us that in general the size of things increases with their weight. The actual size of the heavier box would be so much less than we anticipated and so much less than we had prepared the hand to grasp that by contrast it would seem smaller than it actually was. Its size would be underestimated and for like reasons the size of the lighter box would be overestimated.

We have said that the actual illusion found by these experiments in some respects is, and in some respects is not, the converse of the weight-size illusion. It is the converse of it in this, that the variation of weight has an influence upon the judgment of extent: whereas in the weight-size illusion the variation of size has an influence upon the judgment of weight. It is not the converse, however, with regard to the exact character of this influence. In the weight-size illusion it is found that to increase the size tends to the apparent decrease of weight, and that to decrease the size tends to an apparent increase of weight. The converse of this would be the apparent increase of size with the decrease of weight and the apparent decrease of size with the increase of weight. But we have found by these experiments that in most cases the decrease of weight apparently decreases the size and the increase of weight apparently increases the size. But in one case increase in weight seems to decrease the apparent size and in two other cases to decrease the weight seems to increase the apparent size; so that in three instances we have to a greater extent the converse of the weight size illusion; while in all the other cases the results are the converse only to the extent above stated.

Insofar as these results are not the exact converse of the weight-size illusion their departure is perhaps to be explained as follows: The method or way of judging here was unquestionably different from that employed normally in the weight-size illu-

sion. In the weight-size illusion the heavier box is estimated as larger, in advance of actual touch, and the hand is prepared to clasp it as such; but, as felt—in contrast with what was estimated as its size—it feels much smaller than it otherwise would; whereas in the illusion herein described the method is different. The estimate is not made in advance of touch but as its result. The heavier block is estimated as larger than it really is because it is ingrained in us that volume increases with weight. The lighter block is estimated as smaller for like reasons.

The reason for the illusion disclosed by the present experiments is that experience or custom has taught us that in general the weights of things decrease or increase with their volume. This has become so ingrained that we are influenced when the weight is increased to say that there is also an increase in the volume, although the volume remains constant; and when the weight is decreased, to say, that the volume is decreased also, the volume remaining constant.

In the cases of subjects S and W where an increase of apparent size tends to decrease the weight the probable explanation may be found in a failure to find in the object the size which was anticipated because of the decrease of weight; and the result by way of contrast and by a sort of disappointment was an apparent increase of size.

ON THE TRANSFORMATION OF MEMORY CONTENT IN THE COMPARISON OF LIGHTS.

By T. A. Lewis.

The object of the experiment was to determine how the comparative judgment of two successive light stimuli is affected by the time interval between the stimuli and by the intensity of the standard stimulus, i. e., by the absolute brightness.

G. M. Stratton[1] refers to work, done by students in the psychological laboratory of the University of California, indicating that a sound or light seems in memory to be now more and now less intense than a sound or light of actually equal intensity given afterwards.

J. H. Leuba[2] by his experiments with artificial stars, has made prominent the idea that the behavior of the memory content depends in part upon the quantity of the standard light, finding that there is a tendency for the subjects' estimates to approach the mean of all the different intensities previously used in the experiment; that the light used as standard when at an extreme—either over-strong or over-weak—is shorn of its excess in the judgment of comparison. If the standard was relatively too dim, the second light, to seem its equal, had to be somewhat brighter; and if relatively too bright, the second had to be somewhat less intense.

Baldwin, Warren, and Shaw,[3] in experimenting with classes of college students by means of square figures, to determine the effect of time on memory for size, found that there was a continued increase in the memory estimate of the standard square directly with the increase in time.

[1] Experimental Psychology and its Bearing on Culture, p. 174.
[2] Amer. Jour. of Psy., Vol. V., p. 370.
[3] Psychol. Rev., II, 236.

Lehmann[1] writing on sound found that the judgment of comparison between the absolute and the succeeding variable gave proof that the memory content was minimized by time; a weaker second sound being considered equal to an earlier sound with which it was compared. This experience of Lehmann's was paralleled in another sense-field by F. Kennedy, who, experimenting with pressure sensations having a time-interval of three seconds intervening between the two stimuli compared, found evidence that the quantitative change in memory estimate was towards the limen.

These references, representative of the effort put forth along the line of my investigation, give an idea of its motive and purpose.

In working at the problem here reported, the following apparatus was used: a gas-flame shut in by a sheet-iron hood having a square opening of 50 mm. in front, which allowed the light-rays to pass out and directly through a lens that focused them on a small plate of ground glass covering a hole 9 mm. in

Diagram of arrangement: F, gas flame; B, opaque box: L, lens; S′, first screen; D, disk of episkotister; S″, second screen; E, eye of observer.

diameter in an opaque screen (S′). The size of the image focused on the screen was such that the 9 mm. opening occupied

[1] Phil. Studien VII, 169.

a position safely remote from the flickering edge and the unstable blue core. In this way the maximum of constancy of illumination of the translucent opening was obtained. The standard intensities were fixed by four diaphragms with square openings in size, respectively, 3 mm., 12 mm., 24 mm., and 48 mm. The standard in use was fitted to the lens. One meter from S′ was a second opaque screen S″ that served to conceal from the subject the nature of the changes made and also to cut off from him scattering light rays. In the center of S″ was a hole 19 mm. in diameter, that could be opened or closed by means of a small square of card-board attached to one end of a pivoted rod within reach of the experimenter. In the path of light when it threaded the two screens was a Marbe Rotator arranged as an episkotister whose angular openings had a range from zero to one hundred and eighty degrees. The experiment was conducted in a room darkened except for scattering light from the apparatus. From fifteen to twenty minutes were allowed the subject for adaptation, and at the end of such time he could make out the profile of the experimenter and in a general way the surrounding objects. For some moments after entering the room the darkness seemed absolute. To give light for the operator, I used a single two-candle power electric light enclosed in a card-board box with an opening beneath, directly above the reading scale of the episkotister. An electric clock, connected with a 'second' pendulum, was used to count off time for the experimenter. The subject sat so that his left eye was one meter from S″ and he was fixed in position by a head-rest. The episkotister was always started long enough before exposing the light, to attain a speed exceeding the flicker limit. Whether the light was to be standard or variable was made known to the subject about six seconds before exposure; and then, just two seconds before giving the light, that the subject might get into a proper frame of mind, I gave the signal, 'ready.'

The episkotister was always set at 90° opening for the standard light. The tables show that there were twelve 'variable' lights, and—since two time intervals were used—twenty-four different judgments of comparison. Which vari-

able would be used and what interval—two minutes or two seconds—was decided entirely by lot. The subject was not told what the time-interval would be, and so always expected the second light two seconds after the first, knowing that in case it failed to appear in two seconds the interval would be two minutes.

The experiment was carried out on three subjects, and was therefore necessarily very slow. Only six complete determinations for each of the four standards were obtained from subjects D and B. From subject S two determinations were obtained for the two lowest standards (St. 1 and St. 2), and three and four for the other two (St. 3 and St. 4).

The results are given in Tables I, II and III, being in order the results from subjects D, B, and S. In these tables, directly beneath the heading representing the different standards (St. 1, St. 2, St. 3, St. 4, given in an order of increasing intensity) are the number of judgments, divided into two groups, 'dimmer' (less intense) and 'brighter' (more intense), given for each variable compared with each of the four standards and for the two different time-intervals. The column of judgments 'dimmer' is headed with L (less intensity) and the column of judgments 'brighter' with G (greater intensity). The judgments 'equal' or 'same' were divided between L and G.

The variables are given in degrees of angular opening of the episkotister.

The deliverance of this investigation on how the comparative judgment of two successive light stimuli is affected by the interval between the stimuli seems to be that the judgment is led to underestimate the light that sensation entrusted to memory. A line run through 90° in the table would cut all the variables of standard intensity. Accurate memory would have balanced at this line the judgments L and G for any one time-interval and standard intensity. But subject D makes the equal division (the transition line from a majority of L to a majority of G estimates) of five out of the eight columns below this line (as indicated by short black line); subject B of six out of eight; and subject S puts the transition point of all

eight below the 90° line. Subject S's results cannot be given as great weight as the other results, he having had scarcely half as many 'sittings' as subjects D and B.

In regard to the way the memory-estimate is affected by the intensity of the standard stimulus, the results seem to give some (but by no means uniform) evidence that accords with the theory proposed by Leuba, viz, that a light sensation passing through memory shortly after kindred sensations, is deflected toward

TABLE I.

Subject: Dr. Dunlap.

Variable	St. 1				St. 2				St. 3				St. 4			
	2 min.		2 sec.		2 min.		2 sec.		2 min.		2 sec.		2 min.		2 sec.	
	G	L	G	L	G	L	G	L	G	L	G	L	G	L	G	L
15°		6		6		6		6		6		6		6		6
30°		6		6		6		6		6		6		6		6
45°	½	5½		6	½	5½		6	½	5½		6	2	4		6
60°	1	5	½	5½	3	3		6		6		6	3	3		6
75°	2	4	2	4	4	2	½	5½	2½	3½	1½	4½	5	1	2	4
90°	4	2	2	4	5	1	1½	4½	4½	1½	2	4	4	2	4½	1½
105°	5½	½	3½	2½	4½	1½	2½	3½	5	1	3½	2½	5	1	5	1
120°	6		3	3	6		4	2	3½	2½	3	3	4½	1½	4½	1½
135°	5	1	5½	½	6		5	1	4½	1½	6		5½	½	3½	2½
150°	6		5½	½	5½	½	6		6		6		6		5	1
165°	6		5½	½	6		5	1	4½	1½	6		6		6	
180°	6		4	2	5½	½	½	5½	6		6		6		6	

the line of mean intensity of these earlier sensations. How my investigation agrees with this can be seen by comparing the points of transition or balance in the columns of the extreme lights St. 1 and St. 4. For 2-minute intervals subject D with St. 1 puts the balance-point at an intensity between 75° and 90°—thus below the standard height; and St. 4 is discounted for over-brightness by being estimated still lower, namely, as equal to a variable of 60° opening. Subject B for long intervals as

well as for short has the balance-point apparently a little higher for St. 1 than for St. 4; while subject D for short intervals puts the balance point for St. 1 well above the standard and for St. 4 below the standard. On the other hand, with both D and B, the estimates for St. 3 are about the same as for St. 1.

The point brought out by the students in the Psychological Laboratory of the University of California, namely, that in the

TABLE II.

Subject: Dr. Burrow.

Variable	St. 1				St. 2				St. 3				St. 4			
	2 min.		2 sec.		2 min.		2 sec.		2 min.		2 sec.		2 min.		2 sec.	
	G	L	G	L	G	L	G	L	G	L	G	L	G	L	G	L
15°		6		6		6		6		6		6		6		6
30°		6		6		6	1	5		6		6	½	5½	½	5½
45°		6		6	1	5	½	5½	1½	4½		6	1½	4½		6
60°		6		6	2½	3½	1½	4½		6	½	5½	3	3	1½	4½
75°	2½	3½	4	2	4	2	3	3	2	4	2	4	2½	3½	2	4
90°	3½	2½	3	3	3½	2½	2½	3½	4½	1½	4	2	3½	2½	5	1
105°	6		3	3	3½	2½	3½	2½	4½	1½	4	2	4½	1½	4	2
120°	5	1	3½	2½	5½	½	4	2	5	1	4	2	5½	½	4½	1½
135°	4½	1½	3½	2½	5½	½	4½	1½	5½	½	4	2	4½	1½	5½	½
150°	4½	1½	5	1	5	1	5½	½	5½	½	5½	½	5½	½	6	0
165°	6		5	1	6		6		5½	½	5	1	5½	½	5½	
180°	5	1	5	1	6		4	2	6		6		6		6	

case of sound and light there is a magnifying of the memory estimate for an interval of two seconds immediately following the sensation, finds a parallel in the results from but one observer in this experiment. Subject D's judgments for the short (2-seconds) interval show an elation of the memory content three times out of the four, the point of balance (where the variable light seems to equal the standard) being raised to 105° and above. But subject B's and subject S's judgments show no similar swelling of the memory estimate.

Finally, aside from this working along the investigations of others, there was another phase in memory whose nature I wished to investigate. This phase might be called the range of inconstancy and non-sustaining of earlier judgments (the same stimuli for comparison recurring); the place where the operator is never certain what the subject when comparing the variable with the earlier standard is going to answer. One would think that the memory after a long interval would upon

TABLE III.

Subject: Mr. South.

Variable.	St. 1.				St. 2.				St. 3.				St. 4.			
	2 min.		2 sec.		2 min.		2 sec.		2 min.		2 sec.		2 min.		2 sec.	
	G	L	G	L	G	L	G	L	G	L	G	L	G	L	G	L
15°		2		2		2		2		3		3		4		4
30°		2		2		2		2		3		3		4		4
45°		2		2		2		2		3	1	2	1	3	1	3
60°	1½	½		2	½	1½	½	1½	1	2	1	2	3	1	½	3½
75°		2	½	1½	½	1½	½	1½	2	1	½	2½	1	3	2	2
90°	2		1½	½	1½	½	1½	½	2	1	2½	½	3	1	3	1
105°	2		2		1	1	1½	½	1½	1½	2	1	4		3	1
120°	2		2		1½	½	1½	½	3		2½	½	3½	½	2½	1½
135°	2		1½	½	2		1½	½	2	1	2½	½	3½	½	3½	½
150°	1½	½	2		1	1	1½	½	3		2	1	4		4	
165°	2		2		1	1	2		3		2½	½	4		4	
180°	2		1½	½	2		1½	½	3		3		4		4	

successive occasions be more apt to fluctuate in recalling a sensation than it would after a short interval, and would consequently extend farther the bounds within which the comparative judgment reverses earlier decisions.

But the data of my experiment seem to give no support to this conclusion, there being virtually the same width to the zone of variable judgment for the two-second interval as for the two-minute.

The number out of the twelve variables that each subject puts in the range of error are—giving the average for the four different standards:

Two-minute interval,			D 7 ;	Two-second interval,			D $6\frac{1}{2}$
"	"	"	B 8 ;	"	"	"	B $8\frac{1}{2}$
"	"	"	S $4\frac{3}{4}$;	"	"	"	S 7

THE ILLUSORY UNDULATION AND SHIMMER OF STRAIGHT LINES.

By Prof. George M. Stratton.

It has long been known that when fine parallel lines are viewed from a distance they appear to be irregularly beaded and bent.[1] In 1858 Bergmann, in his account of the matter, tells us that because of the illusory distribution of light and dark many of his observers mistook the direction of such lines, giving them a trend at right angles to their actual direction. And he attributed the whole phenomenon to the structure of the retina, whose elements acting as "visual units" (*Seheinheiten*) introduced an impurity into the spatial perception of the lines.[2]

Helmholtz's well known discussion of the facts[3] is in essential harmony with that of Bergmann. Helmholtz felt that the wavy or beaded appearance of the lines depended upon bringing them close together, and his measurement of the visual angle subtended by two such lines corresponds fairly wellwith the diameter of the elements of the fovea. From this he concludes that the fine distortions of the parallels are due to the mosaic structure of the retina, wherein each element acts more or less as a distinct unit.[4]

Objections to Helmholtz's view have been adduced by Von Fleischl. He showed[5] that the arrangement which Helmholtz regarded as an essential condition of the beading or distortion—namely, that the lines should be so close together as to fall upon adjacent retinal elements—was of no importance whatever;

[1] This was noticed at least as early as 1819, by Purkinje. Cf. Helmholtz: *Physiol. Opik.* 2d Ed., p. 258.

[2] Bergmann: "Anatomisches und Physiologisches über die Netzhaut des Auges," *Zeitsch. f. rat. Medicin*, Series III, Vol. II, p. 83.

[3] *Physiol. Optik.*, 2d. Ed., p. 257.

[4] See also Hering, in Hermann's *Handbuch*, Vol. III, p. 153.

[5] Von Fleischl: "Physiol.-opt. Notizen, II Mitth." *Sitzungsb. d. kais. Akad. d. Wiss. z. Wien*, Math.-Naturw. Cl., Vol. LXXXVI, pt. III (1882), p. 8.

the waves occur quite as well with wide lines at a considerable distance apart as with fine lines close together. The most favorable condition for their occurrence is with lines moving across the retina—the eye looking fixedly at an immovable point past which the lines are being continually carried by the revolution of a drum. Careful measurement of the length and height of these subjective waves showed that they were many times too large to be caused by the mosaic formation, to which Helmholtz had ascribed them. Von Fleischl moreover showed that the aberration in the lines could hardly be due to the network of capillaries in the retina. But beyond this negative criticism, he offers no confident account of the phenomenon, although he suggests that the large epithelial cells might, after a generous allowance for error in his own measurement of the undulations, be large enough to cause indirectly the distortion of the lines.

It seems possible to advance beyond the point where Von Fleischl's excellent observations leave us, and to that end some farther experiments are described, with an attempt to indicate their meaning.

A most significant fact in connection with the sinuous appearance of straight lines[1]—and one that, so far as I know, has hitherto escaped observation—is that the undulation is not visible upon first exposing the rested eye to the lines. It makes its appearance only after some moments of gazing at them, and gradually increases until a maximum effect is attained, this maximum effect continuing thereafter indefinitely. It is difficult to give exact numbers to express the time when this effect begins, but in my own case there seems to be no appreciable waviness until 7 to 10 seconds have elapsed; and often the effect is not fully developed until after two or three times such a duration. These measurements were taken with a group of seven black parallels on white paper, the lines being about 0.5

[1] There is probably some variety in the exact character of this phenomenon with different observers; with some it seems best described as 'undulation,' with others as 'beading.' One of my most careful observers describes the gradually on-coming effect as similar to the annular 'knots' on bamboo. For convenience' sake, then, let any one term stand for all these variants.

mm. wide and 15 cm. long, with centers 2 mm. apart, and observed from a distance of slightly over a meter.

This delay in the on-coming of the undulations may in another way be brought out perhaps more strikingly. If one looks at a system of parallel lines like those just described, continuing his view of them until the appearance of waviness is well developed, and if then the whole system of lines be suddenly revolved about its own center and in its own plane through an angle of 20 or 30 degrees, the waviness is noticeably absent from the lines as they reach their new position, and they must again be viewed for some moments before it returns. A like suppression of the wavy or tortuous appearance may be produced if, instead of revolving the paper, the observer simply turns his head slightly to one side—turning it upon an axis made by the line of sight. But when one says that in changing the relation between the eye and the system of lines the waviness suddenly subsides, this statement applies with entire exactness only under a slight reservation. The waviness entirely disappears only from that portion of the field of view corresponding to the parts of the retina unstimulated by the lines in their former position, but stimulated by them in their later position. Where the lines in their new direction cross the territory occupied by them before, an uncertainty and disturbance of their definiteness is still apparent, undoubtedly as a vestige of their former effect.

There are other circumstances under which the phenomenon here discussed appears or disappears; and these ought to be recounted, as helping in some measure to our better understanding. The phenomenon seems to have no intimate connection with movement of the eyes. It occurs with a free wandering of the line of sight over the lines. And yet the roving of the eyes to an appreciable extent over the lines does not appear to be necessary to produce the waviness; the aberrations occur when the ocular fixation is as perfect as is practicable by merely voluntary control,[1]—although, as we know from mech-

[1] Herein I am running counter to Von Fleischl's statement that the waves disappear with strict fixation (see p. 11 of his article cited above). Can it be that upon strict fixation he slightly moved his head? This, as I have pointed out, would suppress the appearance temporarily.

anical records of the eye's behavior, such fixation is always in a measure wandering and unsuccessful. The phenomenon, occurs, however, when the gaze is fixed to such a degree that a sharp after-image of the lines is obtained. And it is interesting to note that the nicking, the fine distortion, of the lines, is apparent in such an after-image—indeed there is some temptation to say that it is here even more pronounced than in the original perception. Such an observation is perhaps connected with the fact observed by Von Fleischl, that exact accommodation of the eye to the lines is not needed to bring out the waviness—which comes out perhaps more clearly with imperfect accommodation. I find, too, that if ground glass is placed before the lines, but just near enough to permit the sight of them though blurred, the undulatory effect is, if anything, heightened. Some slight blurring, then, is perhaps the vital thing in a number of conditions otherwise so different—where the waviness appears with the interposition of ground glass or with inexact accommodation or in the after-image of the lines. There is no strong reason to think that the blurring is potent only—as one might suppose—by enlarging the opportunity for false observation and for suggestion. The observed effect seems to be too definite and sensuous and unmistakable to be so explained.

The nicking occurs in foveal vision, as Von Fleischl noted in his experiments to determine whether the retinal capillaries were anywise responsible. But this foveal occurrence proves, too, that the waves have no exclusive connection with the retinal rods. Nor is it necessary to view more than a single line in order to produce the undulation. The distortion with but a single line is less striking than when other lines are close at hand; and a longer time seems needed for full development of the waviness. This may be partly because the neighboring lines by their close proximity make more evident the distortion; or to some extent there may here be a psychic summation-effect, the distortion in a number of lines seeming to be a greater distortion in each line, by a very natural associative confusion. But in all probability a number of lines viewed at once act as reinforcing stimulants upon the retina, a given set of retinal elements being more frequently and more rapidly aroused by a

group of fine lines than by a single line, since in the eye's inevitable wandering it tends to pass rapidly from one to another. Yet this oncoming of the undulations with but a single line may the more readily escape an observer because now the same part of the retina must be held with some degree of exactness to the immediate neighborhood of the line. A slight shift of the direction of sight so that the after-image of the line no longer hovers on the border of the line itself but becomes slightly separated from it, causes an instant straightening of the line, while the return of the eye to its old position again brings out at once the nicking.

No markedly different result is obtained when, instead of having a black line on a white ground, the relation is reversed and a white line, or group of lines, is drawn on a black surface. Nor can I notice any decided change in the phenomenon when the black lines are drawn, now on white and now on gray paper of different degrees of darkness. Taking (to be more explicit) the four gray papers of the Hering series of fifty, marked respectively Nos. 1, 15, 30, and 45, where as the reader will recall, No. 1 is white and No. 45 a dark gray that approaches black (No. 50), and drawing upon each of these the group of parallels described earlier, the lapse of time before the waviness appears, is given in the following table:

No. in Hering Series	Nicking appears in about
No. 45	14 Secs.
" 30	10 "
" 15	12–13 "
" 1	7 "
" 1	8 "
" 15	9 "
" 30	10 "
" 45	7 "

After each experiment in the above series the eyes were rested for two minutes, save that at the close of the series there was a rest of half an hour before beginning the series recorded

in the table following, where, it will be seen, the beginning and close is with No. 1 (white) instead of with No. 45, as in the earlier series:

No. in Hering Series	Nicking appears in about
No. 1	14 Secs
" 15	11 "
" 30	8–9 "
" 45	6–7 "
" 45	6 "
" 30	7 "
" 15	13 "
" 1	12 "

The experiment indicates no clear difference between the white and the dark gray; indicates that the position in the series is of more consequence than the character of the ground upon which the lines are drawn, and that in all probability the effect of the preceding trial frequently outlasts the two-minute pause and is carried over into the trial following.

With moderate adaptation of the eyes to darkness (10–12 minutes) the waviness was still observed in looking at fine parallel lines—parallels produced by diffused daylight coming through finely slitted black bristol board inserted in a closely fitting window screen of an otherwise darkened room. And when fairly pure red light came to the eye by placing red glass before the lines—or green light (which, however, was far from spectral purity)—the waviness of the lines seemed nowise affected by the change. The failure to notice any marked influence from the exclusion of rays not red or from adapting the eye to darkness would be in keeping with the observation that the linear undulations occur in the fovea as well as in the eccentric regions of the retina, and consequently have no intimate and exclusive connection with the rods.

If but a single eye be exposed to a set of lines until the undulations are markedly present and then this eye be covered while the other eye is exposed to the lines, there is at the moment of

transition no such vivacity of undulation to be observed as there would have been had this second eye been uncovered while the first eye was exposed. Indeed it is at times exceedingly difficult to notice any transfer of effect. If any result whatever is produced in the second eye, it seems at most to be but a reduction of the time required to produce in full measure the linear undulations there, a noticeable interval still being necessary before the waviness is clearly present.

Passing now from the observed facts to the question of their origin, it is evident that the explanations hitherto offered are far from satisfactory. The earlier account—that the distortion was due to the mosaic arrangement of the retinal elements—would seem already to have been disposed of by the critical experiments of Von Fleischl. Yet Von Fleischl, by suggesting that the undulations were caused by the pigment epithelial cells, because their size roughly corresponded to the size of the linear waves, seems likewise to have been looking in not exactly the right direction, for he too seeks his explanation in some disturbance caused by the fixed structure of the eye's tissues. The present experiments, however, by making clear the *gradual rise* of the undulation indicate, it would seem to me, that the phenomenon is not due to some unchanging condition of the eye such as would be found in its permanent anatomical arrangement for were this the case the undulations would be present as soon as the lines were seen. There must be, rather, some obscure functional change, gradually brought on by continued linear stimulation; and not until this change has reached a certain degree of development does it seriously affect the spatial character of the visual field.

In ascribing this peculiar linear aberration to some functional disturbance of vision, it seems to me that perhaps all has been said that is possible at present with security. Yet beyond this come intimations both negative and positive that may be looked at until the weight of farther evidence seals their fate.

The clearness and distinctness of the undulations seems to me to dismiss almost at once the suggestion that the sinuous appearance is a mere vagueness due to flagging attention. Not only have these waves a direct sensuous vividness, but they are

noticeable only with close attention, and can hardly be due to a failure of careful notice. Nor does it seem probable that these undulations are due to some minute and irregular change in the refractive media of the eye, although changes of such a character might well account for the fact to be explained. Nor is it at all likely that the linear stimulation produces any transitory displacement of the retinal end-organs, after the manner of those more lasting disturbances which—for example, in choroiditis—give a broken and irregular appearance to straight lines. It is, however, distantly possible that a persistent stimulation such as has been described in the present experiment might produce a gradual and slight change in the local signs arising from certain elements or groups of elements either in the retina or in the cortex, and thus there be an altered localization of the dominant visual impression to which the signs were attached.

But rather than give much weight to a suggestion of this sort, it would perhaps seem preferable to believe that the functional disturbance involved in the present illusory undulations was to be described as an irregular and shifting distribution of areas of inhibition and heightened sensitivity. The unusually persistent and teasing stimulus from the juxtaposed lights and darks may perhaps bring on, with time, a momentary local blindness over certain minute areas, and an increased sensitivity in immediately adjacent parts, and for this cause the sensory response would not coincide with the exact outline of the image. Such a shifting distribution of minute induced areas of heightened and depressed sensitivity would in some respects have kinship with the phenomena of fatigue and the passing blindness from long fixation, but it would also in many ways be distinct from either group of facts.

Thus far the account has been confined to the behavior of stationary lines, and we might now consider the appearance of moving lines while the eyes themselves are motionless. The lines used in the experiments to be described were straight and were upon paper fitted to a revolving drum and lay parallel to the axis of rotation. Any particular set of lines had a uniform width and distance apart; but in different sets the width

chosen ranged from about ¾ of a mm. to 15 mm., the distances from center to center varying from 1 cm. to 1.5 cm. In general the lines were black, drawn upon a white ground, although white lines upon black, and black lines upon red, were also used. But unless an explicit statement to the contrary is made, it will be understood that the experiment was with black upon white.

If, now, the eyes be fixed upon a point just in front of the drum while this moves at a speed of one revolution in about 4 to 6 seconds, there is at first no marked effect; but with longer stimulation the lines begin to show those irregularities that appear with motionless lines. It was Von Fleischl's opinion that the movement of the lines while the eyes were fixed upon a motionless point, offered an even more favorable condition for observing the waviness of the lines than did the arrangement where the lines were not in motion; and there seems little reason to doubt the correctness of his statement. There appears here, however, the same delay in the oncoming of the disturbance, it reaching no clearness and force until the stimulus has played upon the eyes a few seconds. But once the eye has been exposed to the stimulus for a length of time sufficient to produce a decided undulation, then even after a moderate rest the effect returns almost instantly upon reëxposing the eye to the moving lines.

A most convincing demonstration of the delay in the coming of the waviness is obtained if one seats himself before the revolving drum so that the eyes are at a distance, say, of 1 m., and, after holding the eyes upon the fixation point for 15–20 secs., suddenly bring them to about 20 cm. from the drum, the ocular fixation being carefully maintained. There is then seen a small central area upon the drum—corresponding to the retinal area already stimulated when the eyes were a meter away—where the nicking and bending appear with great vivacity, whilst round about this central area the lines are still straight and sober. The fresh portions of the retina, suddenly brought to bear upon the lines as the eyes come close to the drum, receive and report them without distortion and give a view in strong contrast with that afforded by the adjacent regions that have

long been impressed by the passing lines. And as with stationary lines, so with moving: a slight rotation of the head upon an axis represented by the line of sight causes an instantaneous reduction, if not an entire suppression, of the undulations, by giving the movement of the lines an entirely new direction relative to the eye. By slowly pendulating the head upon this axis, and thus preventing a continued stimulation in any single direction, the linear waviness can be warded off almost indefinitely. Whether such reduction is quite so great when care is taken that the second motion of the lines, to which transition is made, be the *exact reverse* of the first—by glancing into a mirror that suitably reflects the original,—I am uncertain. But in any event it would seem that the mere interruption of light upon a given point, or movement *überhaupt* over such a point, were not of itself the sole and sufficient exciting factor in this phenomenon; but that the *direction* of movement had its peculiar influence upon the sensitive surface, and that the special retinal effect of a motion in one direction could not be built upon by a motion in some other direction, but this must work up its own peculiar sensory effect *de novo*.

But when the lines are moving before the motionless eye, there is an odd appearance distinct from anything I have described so far. The lines not only appear nicked and bent, but there is also a quiver or shimmer over all the lines; they seem to progress with an odd vibratory movement, that is pronounced enough in observation but is exceedingly difficult to describe. The nearest analogue to it in ordinary experience is perhaps the rhythmic and sometimes disagreeable commotion that runs over the face of a paling as we pass by, when behind it there is a second paling parallel with the first and not too far removed. The passage of the one system of lines over the other, due to the shifting projection as we move, seems to set the whole into a flutter not unlike what is noticeable with motionless eyes before a single system of moving lines.

And this shimmer or flutter, wherein often there is a slight appearance as though the lines, in moving forward, also periodically leapt backward each to the one following on its heels, requires usually some time to develop. With lines about $\frac{3}{4}$ mm.

in width, 1 cm. apart, and the drum revolving once in 7½ secs. (so that 51 lines pass before the eye each second) the shimmer or shiver is usually apparent to me after 7 or 8 secs., and in the course of the next 3 or 4 secs., has reached a considerable vivacity. The time required for the rise of this appearance of vibration seems therefore to be at least roughly the same as that required to produce the distortion of moving lines. And, moreover, like this distortion, the shimmer or flutter is very much decreased, if not entirely suppressed by turning the head upon an axis represented by the line of sight, or by having a mirror beside the drum, into which the observer may glance and see the reflected lines moving in a direction different from that of their originals, without, however, being an exact reversal.

With regard to the influence which the speed of movement might have upon this shimmer, some observations were made. The black lines or stripes used were 5 to 10 mm. wide, 15 mm. from center to center, 33 stripes passing before the eyes—distant 60 cm.—with each revolution of the drum. When the drum made one revolution in 127 secs. I perceive neither distortion nor flicker of the lines, even after looking at the drum for a length of time that with higher speeds would be ample to provoke both of these phenomena. Increasing the speed so that a revolution takes place in 37 secs., it is doubtful if there is any flicker so long as the eyes preserve strict fixation. At 22 secs. a revolution there is a dim suggestion of flicker, with the undulatory distortion perhaps slightly more clear than the flicker. At 10 secs. a revolution the flutter is less dubious, but is still so vague and dull that in all probability it would escape the attention of one not schooled to observe it. Speeding the drum successively to 6 secs., to 5 secs., and to 4 secs., there is noticeable a marked superiority in the 6-sec. speed over that of 10 secs. There is now a clear though delicate flutter of the lines, while the bending is now perhaps less clear than this flicker. As between the 6-sec. and the 5-sec. speed, there is no great difference in their power to induce our phenomena, which here seem to have reached their acme; for with a speed of 4 secs., while the flicker and bending are vivid and are conjoined with brilliant color-effects well-known to observers of such move-

ments, yet with this higher speed there is no clear gain of vividness,[1] and perhaps some loss. With a speed of 1 revolution in slightly under 2 secs. it is far less easy to observe either the flicker or the bending, and there is a less clear effect also from suddenly turning the head. Yet even at this perplexing rate the old phenomenon is still traceable as an appearance of something *beating up against* the actual motion of the lines.

Some interesting facts come to the surface if, instead of using the lines or strips upon the revolving drum, a more powerful stimulation be obtained by having the light of the sky come to the observer through a large slitted disk of black revolved before a bright window. With slits cut radially in this disk and lying nearer to its center than to its periphery, so that the eye is well shaded from the light of the sky except as it comes through the moving slits, a flutter of the lines is soon observed similar to that noticed with the drum. Possibly it arises sooner than with the stripes upon the drum, perhaps because of the greater violence of the stimulation, but I cannot convince myself that there is any instantaneous appearance of the flutter even with the bright light; some moments of stimulation seem always to elapse before the phenomenon is present in force. But what is peculiar to the appearance under these new conditions is, that the flutter takes on the character of a movement of translation easily distinguishable from the actual movement of the slits themselves. It is as though, along with the circling slits and mingled with them, there were a secondary system of vague flickering bands revolving about the same center as the slits and

[1] At other times this 4-sec. rate has seemed more nearly optimal. When the system of lines, however, is completely changed—lines ½ mm. wide, with centers 2 mm. apart—the flutter is excellent with 1 rev. in 33 secs., and seems to have reached its maximum with a rate of about 1 rev. in 24 secs. This latter rate makes the images of the lines pass a given retinal point at time-intervals of about $\frac{1}{10}$ of a second; the favorable 4-sec. rate mentioned earlier would make the lines (since they were farther separated spatially) follow each other about ⅓ of a second apart. It is not improbable, therefore, that the *time-interval* between lines is of far more importance for the vividness of the phenomenon than is mere *speed of movement*. In the experiments with the lines spatially closer together the favorable speed was only about ⅛ of the speed favorable for the lines more widely spaced; but the time-intervals between successive lines were, in the two cases, much the same.

at the same distance from that center and in the same direction, but at a slower speed than theirs. This movement of the fluttering parts is best seen when the eye is directed steadily to the center of the disk, although it does not disappear when the eye is directed to a stationary point against the slits themselves. And the forward direction of the fluttering movement is, with the special arrangement I used, noticeable when the slits themselves progress at a comparatively slow rate. When the speed of the disk is gradually increased, the apparent circling motion of the flutter becomes slower (although the fluttering itself does not cease, it must be understood); and finally, with still greater increase of the disk's speed, it changes its direction entirely, and now runs *counter* to the actual motion of the disk. This secondary system of back-moving, shadow-like, fluttering stripes is totally unlike the appearance of an after-image of reversed motion; for the apparent motion is, in the first place, more rapid than what is noticed in the after-image, and moreover is evident *along with* the rapid forward-movement of the slits themselves. It is similar in appearance to the shadowy movements that pass across two palings with shifting projection, or over the spokes of a revolving wheel at times when seen against the spokes of a wheel beyond. In general character it is, I think, like stroboscopic movement, and what is perhaps its explanation will later be suggested.

Returning now to the revolving drum with its stripes of dark and light, there seems to be no marked change in the phenomenon if the black stripes be considerably increased in breadth and the white stripes be correspondingly diminished, or if the black stripes be fixed upon a red ground instead of upon white, or be viewed through blue or red glasses, or through a pin-hole iris. And I find no unmistakable evidence that the exposure of one of the eyes to the moving lines until the flutter is clearly present does or does not arouse a flutter in the moving lines to which the other eye is then exposed,—or at least help to arouse this flutter earlier than it would otherwise occur. Experiments at one time seem decisive that such a transfer of influence does to some extent take place, while again the observation is unsettling. The following 'timings' are illustrative of what is to be found here:

Exp. 1. In the left eye flicker develops in 9 secs., when there has been no immediately preceding exposure of either eye to the lines.

Exp. 2. In the left eye, flicker develops in 6 secs. immediately after exposing the right eye to the moving lines for 20 secs.

Exp. 3. In the left eye, flicker develops in 7 secs. with no preceding stimulation of the right eye.

Exp. 4 (after a considerable pause). In the right eye, flutter develops in 9 secs. without preceding stimulation of the left eye.

Exp. 5. In the right eye, flutter develops in 7 secs. immediately after exposing left eye for 20 secs.

Exp. 6. In right eye, flutter develops in 10 secs. immediately after exposing left eye for 20 secs.

The time of the beginning of the flutter is under these conditions particularly difficult to fix with confidence, and an error of 2 secs. could easily be made. But on the whole the 'transfer' seems to be slight, if it occurs at all, and thus there is no sure ground as yet for assigning for this phenomenon either a purely retinal or a purely cerebral process as its physiological correlate.

There are a number of well-known facts with which it would seem theoretically attractive to connect, if possible, the shimmer of lines here described. The appearance known as the 'fluttering heart' might, by its very name, seem kindred to that for which I have so often used the expression 'flutter.' McDougall[1] has shown that there are two distinct classes of facts commonly included under the one term 'fluttering heart,'—a class probably due to the peculiar action of the rods when the eye is adapted to twilight vision, and a class observed in daylight and with foveal vision and consequently to be explained in a different way—perhaps (as McDougall, following Schapringer, holds) by the different refrangibility of the rays for red and blue, with which colors the phenomenon is best observed. It is evi-

[1] "Illusion of the 'Fluttering Heart' and the Visual Function of the Rods of the Retina," *British Journal of Psychology*, I, 428.

dent that the shimmer of moving lines in the present experiment does not belong close to the first kind of fluttering heart, for it does not require adaptation to darkness nor paracentric retinal stimulation. And while there may be some connection with the other variety of fluttering heart—where there is a curious slipping of patches moved in daylight-vision, a slipping that occurs both within and without the foveal area of the field —yet this connection can seem reasonable only when we assume that McDougall's and Schapringer's explanation is wrong and that this second variety of fluttering heart is *not* due to any such difference of refrangibility as has just been mentioned. For the shimmer of lines in the present experiment takes place strikingly well with stripes of black and white, where a difference of refrangibility of rays can play no important rôle. But since even the second class of phenomena of the fluttering heart can also be observed with colorless light in daylight-vision,[1] and therefore seem not entirely dependent on the use of colors of different refrangibility, some kind of kinship here is not excluded.

There is perhaps some intimacy between the flutter of moving lines and the appearance of slow reversal that continues for a time in vision after moving lines have long been regarded—the well-known after-image of motion. The great difference in the two kinds of phenomena, so far as their mere primary look is concerned, encourages caution in affirming any kinship. And this caution is further encouraged by the fact that it is possible to produce the after-image of motion without first arousing any noticeable flutter or undulation in the moving lines that provoke the after-image. Thus with very slow movements of the drum—with a speed, e.g., of one revolution in 37 secs.—I may be utterly in doubt as to the presence of any flicker in the moving lines, so long as my eyes themselves remain immovable. Nevertheless, upon stopping the drum, there is a patent and beautiful illusion of reversal. And even with a still slower motion of the drum—a revolution in 127 secs.—I perceive no flutter or bending of the lines; yet upon stopping the

[1] Cf. Sanford: *Course in Experim. Psychol.* I, 318 ff.

drum there is seen the old viscid flow of the lines contrary to their original motion.

Yet I am not sure that this should deter us from thinking that the moving lines, by their continued excitation of the retina, gradually call forth some process which, in its first intention and while the moving lines are still playing upon the eye, gives them a shimmering and sinuous look, but which later and perhaps in the presence of some obscure restorative process in the retina or in the cerebrum, when the lines have ceased their actual motion, makes them appear to move slowly backward. Some incentive to entertain such a belief is found in this: that if the eye be stimulated by the moving lines during short and controllable intervals of time, there is something like equality between the time of stimulation needed to induce the flutter and the time of stimulation needed to insure an after-image of motion. For example, after the drum has attained a speed of one revolution in $6\frac{1}{2}$ or 7 secs., I expose the eyes to the lines (black stripes 5 mm. wide, white stripes 10 mm. wide, 33 of each to the circumference of the drum which is 40 cm. from the eyes) for 1 sec., and stop the drum as nearly instantaneously as I can. During the exposure itself I notice no flutter of the lines nor bending, neither is there apparent any after-image of motion when the drum is stopped. After a rest of 5 minutes I repeat the experiment, now exposing the eye for 2 secs., and again there is no noticeable effect either during or after the movement. With a like rest each time, the exposure is lengthened successively by steps of one or two seconds, and the observations noted:

1 sec. exposure.	No flutter noticed.	No after-image of motion.
2 secs. "	No flutter noticed.	A mere ghost of after-motion (?) in excentric vision.
3 secs. "	Perhaps a slight suggestion of flutter.	Possibly a shade of after-motion in certain outlying parts of field.
4 secs. "	Slightest flutter in certain outlying parts of field.	Slightest after-mage of motion in outlying regions.

5 secs. exposure	Clear, though slight, flutter.	Clear, though slight, after-image of motion.
	(The flutter and the after-image are clearest in approximately the same regions of the field.)	
6 secs. "	Flutter clearer.	After-image clearer.
7 secs. "	Flutter clear, though perhaps no more so than with 6 secs.	After-image clear, though perhaps no more so than with 6 secs.
8 secs. "	Vivid flutter.	After-image excellent, though not so long-continued as with longer stimulations.
9 secs. "	Flutter fully developed, it would seem.	After-image excellent, but apparently confined to outer portions of field, and soon over.
10 secs. "	No noticeable difference from effect with 9 secs. exposure; flutter clearly spread over *both* center and periphery of vision.	No noticeable difference from after-image of 9-sec. exposure. After-image seems to avoid a considerable central area.
12 secs. "	General commotion; greater than before?	No noticeable increase in after-image.
14 secs. "	No noticeable difference in flutter.	No noticeable difference in after-image.

There would thus seem to be a certain similiarity in the external conditions that arouse the flutter and the after-image: they require approximately the same time for their appearance, and with longer stimulation run a fairly parallel course, reaching in times that are not far different their culmination. Yet the sense of their connection is somewhat weakened by the fact that while they make their initial appearance in approximately the same outlying parts of the field, yet the flutter seems more

ready to encroach upon the central region of the field than does the after-image; and too, by the fact already mentioned that with long-continued stimulation by exceedingly *slow-moving* lines an after-image is noticeable where no sign of flutter appears. Moreover it seems easier to reach conviction that there is at least a partial transfer of effect from one eye to the other in the case of the after-image of motion[1] than in the case of the flutter. If there is any connection at all, then, between the two orders of phenomena—other than that they may arise from the same stimulus and in the same organs—it is still obscure and calls for farther study before one can be sure even of its existence.

The evidence seems more conclusive that a close connection exists between the flutter apparent in a system of moving lines and the peculiar after-effect of a moving light noticed by Bidwell, Charpentier and others, and subjected more recently to careful study by McDougall.[2] Such an after-effect of a single bright moving light in my own case presents an appearance like that of a vaguely barred banner of light trailing behind the original impression. And this barred banner does of itself, too, suggest a certain flutter, doubtless because its extreme end is rapidly alternating between phases of dimmer and brighter intensities, and the fold-like series of lights and darks seems to pass over its surface, as in a flag rippled by the wind. But this is noticed only with a comparatively bright light, and not with any such mild stimulus as is given by a dark line upon white paper in moderate illumination. Yet it is highly probable that every patch of brightness, however faint, does when traveling across the retina, leave such an after-effect, either as a low and almost liminal banded sensation, or (if still weaker) as a mere trail of varying *sensitivity*, corresponding to the trail of varying *sensation* with stronger stimulus, but not actually developed into sensation.

Now in either case—whether the trailing after-effect of a

[1] Cf. Von Szily: "Bewegungsnachbild und Bewegungskontrast," *Zeitschrif f. Psychol. u. Physiol. d. Sinnesorg.* Vol. 38, p. 81.

[2] "Sensations Excited by a Single Momentary Stimulation of the Eye," *British Journal of Psychol.*, I, 78; Cf. also, Holt in *Psych. Bulletin*, II, 54, and Nagel: *Handbuch der Physiologie*, III, 222.

weak passing stimulus be a banded after-sensation, too faint and fugitive to be noticed, or be a banded region of contrasted sensitivity—a second weak stimulus in traversing territory so affected would find itself subject to a rapid alternation of reinforcement and inhibition. When the image of a white band falls upon a portion of the retina that (by reason of the stimulation from the preceding white band) is already giving forth a sensation of light or is predisposed to give such a sensation; there is a summation and the brightness is increased; and when the stimulus leaves this zone and passes into one that momentarily is in an opposite phase, there is a subtraction from the proper effect of the stimulus. And thus each line or stripe of the external stimulus used in the present experiment ripples through the wake of its predecessor, and there arises consequently that appearance of flutter over the whole system of lines, so often referred to in this paper.

This hypothesis—that the flutter in a system of moving lines is due to the periodic or vibratory interference between each line of the moving stimulus and the banded after-effect of the immediately preceding line—finds some difficulty in the fact already stated, that the linear shimmer or flutter requires an appreciable time for its development. The peculiar after-effect of a moving stimulus, however, is not known to arise gradually or to require time and something like coaxing, and therefore, the succeeding stimulus (we might expect) would at once find all the conditions needed to produce at once and without delay an appearance of vibration or of shimmer, if this shimmer were actually dependent upon an after-effect like that in Charpentier's phenomenon.

This difficulty is perhaps not fatal. Further observation may disclose the fact that the phenomenon of Charpentier is one whose vivacity increases with successive passage of the stimulus across the same retinal area. My own observations *ad hoc* are indecisive; for while it is true that I do not notice the characteristic banded appearance of a traveling slit until several revolutions of the disk have been completed, yet (since practice is of great importance here) the failure may be due to a certain initial confusion of the attention rather than to a delay

in the appearance of the phenomenon itself. Or even if with the stimulation usually employed in observing Charpentier's phenomenon this effect were to appear at once, this need not mean that with the stimulation afforded by black or white the banded after-effect must reach its acme upon the first passage of the stimulus across a given retinal area. It would be in accordance with other physiological facts if the first after-effect of one stimulation prepared the way for a still stronger after-effect of an immediately following stimulation—somewhat as the excitation of a muscle-nerve preparation not only produces a contraction but also brings on a state of heightened excitability, so that a second stimulation coming in proper relation to the first produces a greater effect than were otherwise possible.[1]

That there is here an interplay of two systems—the images of the actual lines or stripes themselves crossing a retinal region already stirred into minute bands or stripes of excitement and inhibition or of contrasted degrees of excitability—this seems more reasonable when we remember the rate of excitation found to be most favorable for inducing the flutter of moving lines. It will be recalled that this flutter was found to reach its maximum when the lines followed each other at intervals of about $\frac{1}{8}$ to $\frac{1}{10}$ of a second;[2] and this should be compared with the times discovered for the after-pulsations of a single stimulation.

It is evident that the rate at which these pulses occur is various, differing with the strength of the original stimulation, and changing even in a single series of pulses as we go farther away from the initial pulse. With the lower intensities of stimulation by motionless light McDougall attempts no exact report of the time intervals involved, but finds higher up in the series, and with extra-foveal vision, four pulsations in about $\frac{1}{3}$ of a second, and elsewhere estimates that at the close of a series of pulsations from a light of moderate brightness, the rate is about 10 per second, and judges Young's estimate—$\frac{1}{4}$ sec. between the first and second pulse—to be too high.[3] In his experiments with

[1] Cf. Wundt: *Physiologische Psychol.*, 5th Ed. I, 63 f.

[2] See p. 73.

[3] McDougall: *British Journal of Psychology*, I, 85 ff. While the problem discussed above is not the same as that of plotting the curve of the varying inten-

moving lights McDougall found the entire series of primary pulsations to last about $\frac{2}{3}$ of a second, in which time a varying number of pulsations occurred, running at times as high as 18, according to one of his drawings.[1]

Now the rate of movement at which the flutter is found to take place is such as to bring each successive line close enough upon its predecessor to come within its still-active 'wake,' as disclosed by these experiments of McDougall and others. When the lines crossed a given point of the retina at time-intervals of about 1 sec. or more, I was quite unable to be sure that any flutter occurred; while at intervals less than this the flutter gradually became evident—in barest suggestion when the time-interval between lines was $\frac{2}{3}$ of a second; clear at $\frac{1}{3}$ of a second; and clearest at intervals that, under varying circumstances, stretched from $\frac{1}{5}$ to $\frac{1}{10}$ of a second. Since the essential thing would seem to be, that each succeeding line reach the point occupied by its predecessor before the primary pulsating after-effect of that predecessor shall have died away (but hardly that it shall of necessity overtake the very *first* pulse of this series) the times here found seem to meet these conditions and to support the explanation I have suggested.

And this explanation would also cover those special and more occasional appearances—as of shadow-like movements across the face of the system of lines—taking a direction either with or against the motion of the lines themselves.[2] If the present account is correct these shadow-like movements are virtually stroboscopic in character, and are due to the peculiar relation

sity of a sensation arising with different durations of the stimulus, yet it is interesting to note that in this latter case, too, a pulsation of the wave of intensity is found whose first crest may come anywhere between $\frac{1}{5}$ and $\frac{1}{10}$ of a second after the sensation begins, and may be followed by crests at varying later intervals. (Büchner: "Ueber das Ansteigen der Helligkeitserregung," Wundt's *Psychol. Studien* (1906), II, 1; Cf. Berliner: "Der Ansteig der reinen Farbenerregung im Sehorgan," Wundt's *Psychol. Studien* (1907), III, pts. 2 and 3. It would be important if the rate of pulsation in my own experiments and in these others that seem in many ways kindred should be found to be perhaps connected with the 'refractory period' of the cortical cell, which, too, is about $\frac{1}{10}$ of a second (Schäfer: *Physiology*, II, 614 f).

[1] See Plate I, Fig. 6, of McDougall's article just cited.

[2] See p. 80.

of the speed of the lines to the rate of the pulsing after effect. Thus the chief pulsation that intensifies a line as it approaches a given area may find this line occupying a position that is ahead of, or behind, or identical with, the position of its immediate predecessor when it underwent its flutter in that general region. And since this successive advance or retreat is not confined to any single line but is common to them all, there would thus occur at times, in addition to the mere flutter, a wraith-like progress or regression of the entire fluttering system.

SOME EXPERIMENTS ON THE PERCEPTION OF THE MOVEMENT, COLOR, AND DIRECTION OF LIGHTS, WITH SPECIAL REFERENCE TO RAILWAY SIGNALING.

By Prof. George M. Stratton.

The character of the night signals upon our railways has been discussed by the present writer in papers[1] wherein there was offered certain facts of psychology supported by evidence from sources that, it seemed to me, would be regarded as reliable by railway men. There will be no attempt here to repeat all that has thus been published, but since a number of the statements were based upon psychological experiments that could not be fully described without defeating in a large measure the purpose of the earlier papers, it has seemed well to set forth the facts here, so that anyone interested in following the matter may have them in clearer form.

[1] "Railway Disasters at Night," *Century Magazine*, May, 1907, Vol. LXXIV, pp. 118 ff.; "Railway Signals," *Science*, Aug. 23, 1907, N. S. Vol. XXVI, pp. 255 f.; "Railway Accidents and the Color Sense," *Popular Science Monthly*, Mch., 1908, Vol. LXXII, pp. 244 ff. It ought perhaps to be said that in preparing these articles assistance was obtained from two prominent signal engineers, who would doubtless prefer not to be thanked publicly; and use was made besides of such sources as the *Proceedings of the Railway Signal Association*, the *Railway Age*, the *Railway Gazette*, the *Annual Reports of the Board of Railroad Commissioners of Massachusetts*, and a number of other publications of like character. The *Accident Bulletins of the Interstate Commerce Commission* were in constant use, and I am indebted besides to the special assistance of the Secretary of the Commission, Mr. Mosely, and to the Statistician, Mr. Adams. The Pennsylvania Railroad, through Mr. Atterbury, the General Superintendent, very courteously permitted me to make observations from express engines both by day and by night. The experiments here reported would, besides, have been impossible without the kind coöperation of Dr. Dunlap, Dr. Burrow, Dr. Furry, Mr. Hershey, Mr. Barrett and Mr. Williams, who gave their time to tedious observations. Other obligations I shall have occasion to acknowledge in the course of the present paper.

I. As to the Perception of Stationary and Moving Lights.

In considering the possibility of improving the present signals, one readily thinks of using *movements* in the visual field, and especially of using the distinction between movement and rest. And this seems the more inviting since it is a mode of signaling already long resorted to in plain life, as when there is the wish to attract the attention of someone at a distance that cannot well be covered by voice, or when sound for any other reason would be ineffectual. The hand-lantern swung by the trainman shows a ready appeal to this form of signal. And moreover the behavior of animals, many of which are singularly responsive to movements in the visual field (being easily alarmed by moving objects, while utterly negligent of motionless things), seems to give a kind of biological warrant for high hope regarding movements as signs of danger.

Experiments were undertaken to discover how far this hope is justified with respect to very faint lights—to discover whether or in what degree, such lights when in motion are visible at an intensity below that needed for the perception of motionless lights.

In a dark room there was arranged a pendulum provided with a very light horizontal cross-rod running out from a point close to the center of motion. Upon this rod, and at a distance of about 35 cm. from the center of motion, there was carried a small circular white disk, 13½ mm. in diameter, which, upon swinging the pendulum, moved up and down. This vertical movement of the disk was about 10 cm., and through this stretch it oscillated at a rate of 128 single vibrations per minute. The pendulating disk was illuminated from the front by a miniature electric lamp connected with a storage battery—a lamp enclosed in a small opaque chamber screened from the observer and provided with an opening toward the disk. White tissue paper covered this aperture of the chamber, cutting down and suitably diffusing the illumination; while the variation and control of the light, in order to discover the threshold of perception, was effected by an episkotister driven electrically. To give the observer the general direction in which he was to look for

the disk in the dark, there was set at each side of the disk, and distant from it about 8 cm., a solitary phosphorescent spot, immovable and dimly seen when once the eyes were partially accommodated to the darkness. The observer, whose eyes were a meter from the disk—a distance preserved by means of a Hering head-rest—remained in the dark room for at least ten minutes before the beginning of the day's experiments. Such a preliminary stay was also required in all subsequent experiments.

Since the aim, as I have said, was to determine whether a moving object could be seen at a degree of illumination less or greater than that needed to disclose this same object when stationary, the disk was at rest in certain series and in other series oscillating, and measurement was taken of the relative intensity of the light necessary for the two kinds of perception. The series involving motion were alternated in a given hour with those involving rest; and from day to day the order of these different series was changed to reduce the error that might

Series with Moving Disk.		Series with Stationary Disk.	
Opening of episkotister, in degrees.	Observation.	Opening of episkotister, in degrees.	Observation.
10	'Nothing'	10	'Nothing'
20	"	20	'Some intimation, but did not last.'
30	"	30	'Seen for a moment, motionless.'
40	'Mere suggestion of movement.'	40	'Seen.'
50	'Seen.'	50	"
60	"	60	"
60	"	60	"
50	"	50	"
40	'Very indistinct.'	40	"
30	'Nothing.'	30	"
20	"	20	'Nothing.'
		10	"

arise from progressive adaptation in the course of the hour. The observer was left free to regard the disk with foveal or with extra-foveal vision, in the natural manner in which a signal would have to be observed. The table above illustrates the general procedure, and the character of the judgments passed.

The thresholds, expressed in degrees of angular opening of the episkotister, and set down in the order in which they were obtained for any one subject, are given in the following table:

Observer.	Thresholds for moving light.	Thresholds for stationary light.
W	30.	20.
	30.	20.
	50.	20.
	10.	10.
	10.	5.
	5.	15.
	15.	10.
	10.	5.
	5.	10.
	10.	5.
	Average: 17.5	Average: 12.
F	80.	20.
	70.	30.
	40.	20.
	40.	30.
	20.	20.
	20.	20.
	15.	5.
	10.	5.
	15.	10.
	15.	10.
	10.	10.
	10.	10.
	Average: 28.8	Average: 15.8

The table indicates some advantage, on the whole, for the motionless light; it was perceived at a degree of illumination

at which, on the average, the moving light was still invisible. But in the single determinations the motionless light is by no means universally favored; for observer W the *lowest* determinations for the one kind of perception are no lower than for the other, while with observer F the minimal thresholds are on the side of the stationary light. What difference there is thus leans to the side of the moveless stimulation. And in this very fact there is some surprise. The expectation won from the observation of strong lights, which in movement are so attractive to the attention, is not met by these weakest ones. We must say, therefore, that the advantage of moving lights is probably confined to those regions of intensity that are well above the threshold—where the sensation already has some *pou sto* from which to move the attention; their advantage does not affect the threshold of perception itself, the absolute threshold of sensation. Just how far the advantage possessed by the higher intensities is an offset to the want of such advantage, or even to the actual disadvantage, in the lower intensities, remains yet to be determined.

As to the cause of the higher absolute threshold for moving lights, it is probably connected with the wide fact, already recognized, that the threshold of sensation depends not alone on the absolute intensity of the stimulus, but also upon the duration and the area of its operation. Now in the present case the matter of area hardly enters, for at any instant the area of actual stimulation is the same in the two cases. But the motionless light plays upon any given retinal element a longer time than does the moving light, and thus supplements its own physical intensity by this increased duration. Accordingly the weaker intensity is enabled to attain a luminal value when the stimulus is stationary; but when moving, a greater strength is necessary to make good the loss due to the mere brevity of the stimulation upon any given retinal element.

II. As to the Perception of Signal Colors.

With regard to colored lights, the aim of the experiments was to discover the point at which the color used as a sign of danger

ceased to be perceptible, and to compare this with the corresponding point for the yellow light commonly called 'white,' that on many roads is used as a sign of safety. And since other roads use green as a sign of safety instead of using it as a caution signal, the color green was also to some extent included in the experiments. But particularly the purpose was to determine the advantage or disadvantage in the use of the present danger-signal, as compared with white, when for any reason the light is reduced to a very low intensity—by the distance, or the lamp's low burning, or by smoke, dust, snow, or fog. As with the moving and motionless lights, it was an experiment on the absolute threshold of sensitivity, and left out of account entirely the relative attractiveness for the attention of the impressions when once they are strong.

It must be remembered that in actual railway practice the effect of red as compared with white depends not merely on physiological and psychological differences, but on physical. In the red signal there is employed the same flame or other source of light as for white, and the same intensity, together with the same devices for reflection and for concentration of the beam. But for producing a red light, especially in the block system, there is ordinarily passed before the lantern at the proper time a red disk of glass, called a 'roundel,' that absorbs a large portion of the light emitted by the flame for the white signal, and permits the passage only of those rays that, either of themselves or in combination with other rays, give a strong impression of red. But this absorption greatly reduces the intensity of the light, so that the red signal thus at the very start is physically but about $\frac{1}{3}$ to $\frac{1}{8}$ as bright as the light that constitutes the white signal.[1] If one is to drive directly at the problem with which signaling is concerned, rather than at the one which would be of greater interest to abstract psychology, this physical difference must be included among the conditions of the experiment, and not excluded by first producing a red and a white whose physical intensities were as nearly as possible equal.

[1] See Churchill: "The Roundel Problem," presented at the Ninth Annual Meeting of the Railway Signal Association, 1905.

The experiments were conducted both within and without the laboratory, over long and short ranges, with light reflected from disks (as in the preceding experiment), and with light coming direct from lamp to eye. The colors, in all save the very first and preliminary experiments, were obtained by sending a beam of light through standard signal glasses used in the railway service.[1] The limit of perceptibility was found in several ways. In some of the preliminary experiments the miniature lamp with its windowed encasement already described was used to illuminate a small white disk that served as the object of observation. The distance between disk and lamp was varied until, with red and with white light, the just-perceptible intensity was reached. But more commonly the thresholds were found by placing the lamp and its red glass at such a distance from the disk as to make this just visible; and then, by means of an episkotister, after removing the red glass, the far lower intensity of the lamp was found at which the disk, under this 'white' light, could just be seen. But this procedure, for all its convenience, seemed to imitate too imperfectly the conditions of actual service, when the object seen is usually a small area of light having a visual angle far less than that of my disk, and moreover is not a reflecting surface but a beam coming direct from lamp to eye; so the apparatus was rearranged to produce this effect. By means of a system of dispersion lenses there was now obtained from an oil-lamp properly screened a point of light of an intensity so reduced that when the red roundel was placed before the lamp this point of light was barely visible. Then for comparison the roundel was removed; the (now 'white') light was, by means of an episkotister, reduced below the point of visibility; and the threshold was approached and crossed by gradual increase. In still other experiments the dispersion lenses were set aside and an opaque screen with an aperture of 0.75 mm. (0.03 in.) was placed before the sema-

[1] For these glasses I am indebted to the Corning Glass Works, and especially to the vice-president of the company, Mr. Houghton. The use of a railway semaphore-lantern with a special attachment permitting an easy interchange of roundels is due to the courtesy of the Adams and Westlake Company, and more particularly of Mr. Langworthy of their Eastern office.

phore lantern provided with a 'slow-burning' oil-lamp, prism reflector, and white glass lens. This aperture when viewed from a distance of 18 ft. was about equivalent, so far as the area of retinal stimulation is concerned, to the illumination from a signal-lantern having a 5 in. lens-opening observed from a distance of nearly 3000 ft.

In these experiments it soon became evident that the threshold for white and the threshold for red, bore to each other a relation far different from what we should expect from our knowledge of the physical relations of the two lights. Physically the presence of the red roundel of the style I used cut down the light to about $\frac{1}{6}$ of the intensity of the light when no such roundel intervened. From this cause alone, then, we should expect in reaching the threshold to have to cut down the white light by means of an episkotister or other device, to about $\frac{1}{6}$ of that intensity of light which, behind the red roundel, was just perceptible. In fact, however, the white light had to be cut down until the ratio between it and the light just seen through the red roundel was as follows:

Observer F. 1:36
S. 1:24
B. 1:18

These determinations were made within the laboratory and when the light was viewed direct. They do not indicate any thing other than the most favorable observations under the conditions; for with all the disparity between white and red, which these ratios indicate, there came times when the disparity was still greater: with one observer the red light was still invisible when the episkotister was opened 60 times more than was necessary for the white; with another observer an opening 70 times greater than for the white still failed to make the red perceptible. When the object to be observed was not the point of light viewed direct, but was the small white paper disk reflecting the light, Observer F did not see the disk under red illumination until the distance between light and disk indicated an intensity (neglecting the physical absorption

by the red roundel) 70 times greater than that needed for the white light.

A few trials were made at night in the open, the observer being distant about 4200 feet from the light. The air was more or less smoky from a neighboring railroad, thus supplying still more nearly the conditions frequent in actual service. This smoke of course affected the red light far less than it did the white, since smoke is less hostile to the passage of red rays than to those of other colors, and in consequence cuts off a greater proportion of the white light's full intensity. Under these circumstances the ratio of the lights just visible was, for myself 1:14—a reduction of the second term of the ratio below the lowest point found for any of the observers in the laboratory experiments. And yet this means that under conditions of atmosphere that tend to equalize the two kinds of light, the red signal disappeared at an intensity which, upon removal of the red roundel, was 14 times brighter than was called for to render visible the safety signal.

If, instead of white, green be used to indicate safety, this both doeseand does not materially change the relation between the danger signal and the safety signal in regard to their comparative perceptibility. With a brightness of light that at 10° opening of the episkotister the common warm light of the oil flame could be recognized as 'ordinary light' by one of my observers, the interposition of a green roundel did not prevent his having a sensation of light when the episkotister was open 15°, but the light seemed colorless; at 180° opening it was judged to be 'bluish green,' but again at 360° opening it was called 'bluish white,' though still faint. With another subject who at 20° saw the white light when no roundel was interposed, a 50°-opening of the episkotister made him see the light merely as 'light' when coming through the green roundel, while at 80° it still seemed mere 'light;' and even at an opening of 360° it appeared to him only a 'grayish blue.' Under the same circumstances, when a red roundel was substituted for the green, an opening of 360° gave an impression of red distinctly for the one observer, and doubtfully for the other. It is therefore clear that the relation of the red to the green signal can be stated in

two different ways, according as we consider the green in its chromatic or in its achromatic character. Considered in the latter way, the contrast between the perceptibility of the danger signal and of the safety still remains large; but considered in the former way—requiring the green to be recognized as *green*—the inequality between danger and safety almost, if not fully, disappears; indeed it may at times even be transformed into an inequality where the red is superior.

For practical purposes of signaling the green must be recognized in its true chromatic character, and therefore its use to indicate safety tends to reduce or cancel the relative disadvantage of the danger signal. But this simply means that *both* signals are now liable to fail from too low intensity, whereas before only one was peculiarly apt to disappear. The red itself however, has not thereby been made essentially more effective nor its inherent disadvantages overcome.

From our familiarity with the phenomenon of Purkinje, such a relation between red and green and white is not surprising. Yet a more concrete grasp of the facts as they appear under the conditions of color-production in the railway service does give its own stir and shock, and ought in the end to contribute to a better understanding and advance of practical signaling.

III. As to the Perception of the Luminosity and Direction of Lines.

The chief coadjutor of color for signal purposes is direction. By day the enginemen on most roads that use the block system are forewarned of the condition of that portion of the track they are about to enter, by the position of an arm or vane above or beside the way—pointing horizontal or vertical or at an angle somewhere between. Bearing in mind this common use of direction by day, the more distant question with which the present experiments were concerned was this: Would it be feasible to use by night a line of lights that by its direction of pointing would give different indications, in much the same way as that by which the present day-signals convey their meaning?

And toward this end two more immediate and particular queries were propounded:

1. How would the intensity at which a row of lights is perceptible in its proper direction compare with the intensity at which a single light in that row can just be seen?

2. Of what length must such a row of lights be to insure perception; and how does this minimum length compare with the distance from which the row is to be observed?

The first of these questions amounts to this: Would a signal given by the direction of several lights in a row be readily observed? Would its indication be more easily lost in fog or smoke or snow than is that of a single light, such as the present white safety signal, when of an intensity equal to one of the members of the row? The experiment was thus parallel in some measure to that by which the just-perceptible intensity of movement and rest and of the more prominent signal-colors had been studied.

A row of seven white disks, each 13½ mm. in diameter, with centers 15 mm. apart, were mounted on black bristol and so arranged that their line might be at will either horizontal or vertical, and also that six of the disks might be covered, leaving only one exposed. The illumination was by means of the hooded miniature incandescent lamp described above, and the threshold was determined by minimal changes produced by an episkotister. The observer was about 2 m. from the disks, and was left free to look at them without constraint to purely foveal vision; the experiment was conducted in a dark room, and no observations were begun until after at least 10 minutes had elapsed for accommodation.

Thresholds, expressed in degrees of angular opening of the episkotister, were found as follows:

Subject.	Threshold for single disk.	Threshold for direction of series of disks.
S	60	20
	40	10
	Av. 50	Av. 15
F	35	15
	25	20
	10	10
	10	5
	40	25
	40	15
	Av. 27	Av. 15

For each of the subjects the perception of the row of lights and the detection of their proper direction was thus attained at an intensity of illumination far below that necessary for the perception of but a single one of the disks. The comparison of the thresholds indicates that such a series of lights has an advantage over the single light, of about 2 or 3 to 1.

The explanation of this advantage of the series of lights is doubtless to be found in two well-known principles of vision. In the first place the observation of the row is far less likely to be made by foveal vision exclusively than is that of the single spot; and any extra-foveal perception, under the conditions of the experiment, would of course permit a sensation to arise from a lower intensity of stimulation. In the second place the larger area of the retina stimulated, even regardless of any inherent differences of sensitivity, would help to reduce the threshold; since, other things being equal, a larger expanse of stimulus gives a more intense sensation. Moreover in the present observations the threshold for perceiving the *direction* of the line or row was not clearly separate—as might have been expected from observations in other fields[1]—from the threshold for the

[1] For a single example out of many, see "Ueber die Wahrnehmung von Druckänderungen," *Philos. Studien*, XII, 538, where the *direction* of change has a higher threshold than mere change itself.

perception of mere brightness; the one seems to have been noticed almost if not quite as soon as the other. Since, then, our perception of the direction of a luminous band runs relatively so low, it is clear that a night-signal composed of a row of lights would have a far better chance of perception than has a single light of like brilliance. It would be possible to see a row of lights and to be certain of its true direction under conditions that would make a single light too faint to be seen at all —conditions that often come by reason of fog or the smoke of passing trains.

Passing now to the second question, as to the *length* of line needed for the clear perception of direction, it can readily be understood that this is of moment for practical signaling, since with the high speed of our fastest trains the meaning of the signal must be caught from a considerable distance—a distance sufficient to permit the engineman to bring his train to a stop, if necessary, at the signal post. Would the required length be so great that from practical considerations it would be inconvenient or even impossible to use a linear signal instead of the present chromatic signal by night?

The first attempts to answer the question were made crudely with a row of pin-pricks in an opaque circular screen set close upon the lens of a semaphore lantern, the direction of the row being variable simply by turning the circular screen. Under these conditions an observer might, with 4 pin-holes, each a millimeter from its neighbors, measuring from centers (so that the row had an extreme length of about 3 mm.), still detect the direction of the row when his distance from the screen was increased to 3 meters. With a similar row of points, but each point now 2 mm. from its neighbors (so that the row was now twice as long as before) the observer might, though with difficulty, give proper answer as to the direction of the row of points when his distance from the screen was as much as 8 meters.

The experiment was then tried *sub Jove*, in order to have more nearly the conditions of actual railway service. Stations upon opposite sides of a ravine or narrowish valley could conveniently be used, where the distance between the stations was found by triangulation to be approximately 4200 feet. Below

the air-line connecting the stations and along the floor of the ravine ran a railway of heavy and frequent trains that generously burdened the air with smoke. The lights to be observed were incandescent bulbs of 8 c.p., set in a row along a wooden vane, with a distance of 10 inches between the centers of any two neighboring bulbs. Six of these lights were on the vane, bringing the extreme length of their line to 50 inches.

The vane was held in either a horizontal or vertical or oblique direction in irregular sequence until a series containing each of these directions either 3 or 4 times had been completed. Another series was then carried out, with a different length of line, after the manner once described as the Method of Serial Groups.[1] For simply decreasing the length of the row one of the end lights was cut out; at other times, as will be indicated in several of the tables, intermediate lights were also omitted; both the length and the inner constitution of the series will be clear, therefore, if the lights be numbered consecutively from 1 to 6, and the serial numbers of the lights composing the series be actually given: 'Nos. 1, 3, 4, 6' will thus denote a row 50 inches long, with the second and the fifth lights of the series omitted. The judgment of the observer, after each display of the line of lights, was signaled back by means of a code of lantern flashes to the conductor of the experiment, and by him recorded. The following tables give the results, wherein the letters v, h, ob, and d stand respectively for 'vertical,' 'horizontal,' 'oblique' and 'doubtful.'

[1] *Psychological Review*, IX, 444.

OBSERVER H.

ACTUAL DIRECTION OF THE LINE OF LIGHTS.	JUDGMENT OF THE OBSERVER.				
	With 50″ line (Nos. 1–6)	With 40″ line (Nos. 1–5)	With 30″ line (Nos. 1–4)	With 20″ line (Nos. 1–3)	With 10″ line (Nos. 1–2)
v	v	v	v	v	ob
h	h	h	h	–	h
v	v	v	v	v	v
ob	ob	ob	ob	h	d
h	h	h	h	h	h
v	v	v	v	d	h
ob	ob	ob	ob	ob	d
h	h	h	h	h	d
ob	ob	ob	ob	h	d
No. of failures in the series	—	—	—	3	6

OBSERVER B.

ACTUAL DIRECTION OF LINE OF LIGHTS.	JUDGMENT OF THE OBSERVER.					
	With 50″ line (Nos. 1–6)	With 40″ line (Nos. 1–5)	With 50″ line (Nos. 1,3,4,6)	With 50″ line (Nos. 1–6)	With 50″ line (Nos. 1, 3, 6)	With 40″ line (Nos. 1, 3, 5)
v	v	d	v	v	v	d
ob	d	ob	ob	ob	ob	ob
h	h	h	h	h	h	d
v	v	d	v	v	v	d
v	d	v	v	v	v	d
ob	ob	d	ob	ob	ob	ob
h	h	h	h	d	h	h
ob	ob	d	ob	d	ob	ob
h	h	d	h	d	h	d
v	d	v	v	v	v	v
No. of failures in the series	3	5	—	3	—	5

OBSERVER D.

ACTUAL DIRECTION OF LINE OF LIGHTS.	JUDGMENT OF THE OBSERVER.					
	With 50″ line (Nos. 1–6)	With 40″ line (Nos. 1–5)	With 50″ line (Nos. 1,3,4,6)	With 50″ line (Nos. 1–6)	With 50″ line (Nos. 1,3,6)	With 40″ line (Nos. 1, 3, 5)
v	v	d	v	v	v	v
ob	ob	ob	ob	ob	ob	ob
h	h	h	h	h	h	h
v	v	d	v	v	v	v
v	v	v	v	d	v	v
ob	ob	d	ob	ob	ob	ob
h	h	h	h	h	h	h
ob	ob	ob	ob	d	ob	ob
h	h	h	h	h	h	h
v	v	v	v	v	v	v
No. of failures in series	—	3	—	2	—	—

OBSERVER S.

ACTUAL DIRECTION OF LINE OF LIGHTS.	JUDGMENT OF THE OBSERVER.						
	With 50″ line (Nos. 1,3,6)	With 40″ line (Nos. 1,3,5)	With 30″ line (Nos. 1,3,4)	With 20″ line (Nos. 1–3)	With 30″ line (Nos. 1,3,4)	With 40″ line (Nos. 1,3,5)	With 50″ line (Nos. 1,3,6)
v	v	v	v	ob	ob	v	v
h	h	h	h	h	h	h	h
v	v	v	v	v	h	v	v
ob	ob	ob	ob	ob	ob	ob	ob
h	v	h	h	h	h	ob	h
v	v	v	v	ob	v	v	v
ob	ob	ob	ob	h	ob	ob	ob
h	h	h	h	h	h	h	h
ob	ob	ob	h	ob	ob	ob	ob
v	—	v	v	ob	v	v	v
h	h	h	h	h	h	h	h
ob	ob	ob	ob	ob	ob	ob	ob
No. of failures in the series.....	1	—	1	4	2	1	—

From the tables it is seen that observer H was able to judge correctly the direction of the line when it was reduced in length from 50 in. to 40 in. and even to 30 in.; only at 20 in. did failures begin to appear. In his case, therefore, the direction of the line was still perceptible when the length of the line was about 0.0006 of the distance between observer and signal, or when the extremities of the line subtended a visual angle of about 0° 2′. With no other observer, however, was there such nicety of perception; for the most part a shortening of the line to 40 in. was marked by the appearance of failures of judgment. But with a length of 50 in. (under certain conditions of illumination, of which more will be said later) it was always possible to run through a series without errors of judgment. Such a 50-in. line is approximately 0.001 of the distance between the signal and the observer's station, and subtends a visual angle of about 0°3′20″—a result that roughly agrees with that obtained in the preliminary experiments in the laboratory: when a row of pinholes 3 mm. in length could be correctly judged as to their direction from a distance of 3 m. the ratio is 1:1000—; when a row 6 mm. long could be judged from a distance of 8 m. the ratio is approximately 1:1300.

The angular measurement here is well above that obtained for the perception of difference of locality by means of adjacent points or parallels, where the space threshold descends from 60″ to a value as low as 30″,[1] or even to 7″ and lower, under special conditions of experimentation.[2] But the higher threshold in the present experiment is hardly to be wondered at, when one considers how different are the conditions of perception here from those that give us the accepted values of the *minimum visibile*. Doubtless if discrimination of direction with the shortest possible extent of line were the one thing needful in a practical railway signal, far more favorable condtions for catching the direction could easily be devised—probably by using a weaker intensity of light than was here employed, and by reduc-

[1] Helmholtz: *Physiologische Optik*, 2d Ed., pp. 256 ff.

[2] "A New Determination of the Minimum Visible," *Psych. Review*, VII, 429; "Visible Motion and the Space Threshold," *Psych. Rev.* IX, 433. Cf. Bourdon: *La perception visuelle de l'espace*, 144 f.

ing the number of lights to two. But there would, in that event, be the disadvantage that with very weak intensities the signal might in many instances not be seen at all; and with but two lights instead of more, the lights might often appear discrete rather than linear, and the danger would be great of mistaking for a signal two meaningless lights that happened to be in proximity to each other in windows or on the street, especially when near some point where a signal was expected.

Yet to some extent a concession might well be made. Experiments with two of the subjects show that in general the same length of line can better be discerned when given with longer intervals between the constituent lights of the row. With observer B, when the 50-in. line was given by means of all six lights, three errors were made in each of two series. But when exactly the same length of line was given by means of fewer lights no errors were made.[1] That with a 40-in. line as many errors were made by this observer when the line was given by 3 lights as when given by 5, is perhaps due to the fact that for either arrangement the length seems to have been well below the threshold, and consequently no difference could appear.[2] With observer D the advantage of fewer lights is again apparent: in the 50-in. line with 6 lights one series shows no errors, while another shows 2; but when the 50-in. line is given by fewer lights no errors are in either of the two series. And with a line reduced in length to 40 in., 3 errors occur when given by 5 lights, but when given by 3 lights all errors disappear.

It is thus evident that one can over-reach in the very effort to make the line striking by multiplication of lights densely massed. The line then, because of retinal irradiation as well as of faulty refraction, loses nicety of form. The most favorable arrangement thus lies in a happy mean between lights so faint that they are in danger of remaining unseen at some critical time, or so far-spaced that they are seen discrete and not as a continuous line, and a crowded row that blinds the sense of direction by excess of light.

[1] See columns 4 and 6 in table on p. 99, and cp. with columns 2 and 5.

[2] See columns 3 and 7 in the same table, where it will be noticed the answers are only half correct.

IV. Résumé of Experimental Results.

The experiments here reviewed make it seem probable that:

1. A stationary object can be perceived at a lower intensity of illumination than is needed for a moving object. This at least is the case when there is more or less adaptation to darkness—and runs counter to our expectation based upon the evident influence which supra-liminal moving lights have over the attention.

2. The red danger-signal requires for its perception an intensity of light far greater than is needed for a 'white' light, and moreover far greater than can be accounted for by the physical absorption-coefficient of the red glass. The intensities necessary for the red, range from 14 to 70 times that needed for white.

3. In comparison with green, the red danger signal is at no such disadvantage, although its absolute defects, of course, remain the same. Green is perceptible as mere light at an intensity of illumination far lower than that needed for red; but to recognize it as *green* the intensity must be increased until it is almost if not fully that required for red.

4. A number of lights in a row may be perceived, even as regards the *direction* of the row, at an intensity less than the threshold for any one of the constituent lights alone. The advantage of a row over a single light may be as high as 2 or 3 to 1.

5. A number of lights in a row may be perceived in their proper direction under circumstances approximating 'service conditions' when the length of the row is about 0.0006 to 0.001 of the distance of the observer from the row, or (to express it otherwise) when the extreme lights subtend a visual angle of approximately 2′ to 3′. Prudence would of course suggest that in actual service this limit should not be too closely approached.

6. The optimal relation of lights for the perception of direction with a minimal length of line requires a certain moderation of the intensity of the row, either by reducing the number of lights (short of an appearance of discreteness) or by reducing their candle-power.

The bearing of the above facts upon the problem of night-signals for railways I have attempted to set forth in the articles cited near the beginning of the present paper. So that I shall not venture to repeat what has there been said. But apart from any personal interpretation or proposal, the experiments help to make clearer the plain facts that must be taken into account by those responsible for a system of railway signals.